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FOREWORD

THE growth of science during the last three hundred years has brought it in these days into a position of supremacy, both in the determination of the course which our common civilisation is to take in the future and also in the shaping of the philosophy with which the individual thinker seeks to co-ordinate and understand the variety of his experience. The contrast between the scientific activity of the seventeenth century, when our present outlook on scientific problems may be said in a sense to have had its origin, and that of the present day is indeed striking. In the age of Newton, the pursuit of science was almost an esoteric occupation, conducted by a few working in isolation and little influencing or being influenced by the larger movements that made contemporary history. On the other hand, science was seen clearly enough to constitute a philosophy—*natural* philosophy, or *experimental* philosophy—and to be inseparably related to the fundamental problems of the meaning and purpose, if any, of existence : thus, to Galileo mathematics was the language in which the great book of Nature was written, and to Newton space was the expression of the omnipresence of God.

Today the position is reversed. Individual scientific activity becomes less and less conspicuous. For the most prominent scientific investigations workers tend to be harnessed to a programme of research as soon as they are trained, and though produced in numbers that would at one time have seemed impossible, are still too few for the work to be done. Correspondingly the philosophical aspect of science has become obscured ; science is no longer primarily a philosophy but an instrument. Mathematics is the means by which technical problems are solved, and its most complex operations are performed by machinery. In brief, science is in danger of becoming transformed from a vision to a blind automatism, its pursuit from a pilgrimage to a chase.

Now while this can be seen to have been to some extent inevitable, and not necessarily wholly a disaster, it is nevertheless in the highest degree undesirable that the extended application of science to practical affairs should be accompanied by loss of apprehension of its philosophical significance. Such a result, however, would appear inescapable unless a positive effort is made to keep alive the sense of this more fundamental contribution which science can make to the satisfaction of our common human needs. The young student of science has now no leisure to reflect on the meaning of his work. As soon as he has passed through the strenuous undergraduate days he enters a field where the race is to the swift and favour to men of skill who have no time to become men of understanding. Thus there is tending to grow up, in larger and larger numbers, a community into whose hands the reins of civilisation are passing, and in whose minds there is less and less understanding of the motive power which they alone are in a position to control. The probable consequences need no emphasis. Nor is it only over public affairs that the menace hangs. The deep personal problems which the reflective in all generations feel impelled to face, become more and more intertwined with the knowledge that science brings, and it becomes ever more imperative to understand the meaning of such knowledge and its relation to the religious, ethical, æsthetic and other elements of our mental environment. The faster knowledge grows and the more deeply it penetrates, the more difficult becomes the work of assessing its ultimate significance. Only the scientist can do it adequately, and he is becoming less and less capable of the task because the nature of his training and his preoccupation with the exigencies of his immediate calling do not, and are not intended to, fit him for this task. Again the probable consequences need no emphasis.

These considerations have led to the formation of the Philosophy of Science Group of the British Society for the History of Science, of which particulars will be found on page ii of this issue. The Group has now been in existence for two years, and its experience during that time has shown the need for a Journal in which papers presented to it, as well as contributions from other sources, can be placed on permanent record and become available to a wider public than that able to attend its meetings. The present Journal is an attempt to meet this need. It is the mouthpiece of no particular doctrine, and will seek to further no cause other than that of general enlightenment. The members of the Editorial Board—composed, as will be evident, of men holding widely differing views on philosophical, religious, political, and indeed almost every other question—are united in this, that they realise the need for such a Journal as it is hoped to maintain and believe that it can make a contribution of the highest importance to the life of our time.

While the scope of the Journal is necessarily more general in character than that of the Proceedings of the various scientific societies, and consequently should include a larger range of interest with a correspondingly wider appeal, it is not intended that the articles shall be any less precise or more ' popular ' in the ordinary sense of the word than those of the more technical publications. The understanding of science demands not airy generalisation but exact analysis based on intimate knowledge, and the Journal will afford a medium through which serious studies, often of difficult questions with no obvious direct bearing on the larger ultimate issues, can be made accessible to those who do not expect to have their minds made up for them without the expense of thought. Since the whole range of science—physical, biological and psychological—will be the subject of study, it is not to be expected that every reader will find all the articles equally comprehensible. The problems to be discussed do not admit of easy solution, nor indeed is their solution the only object to be achieved. If the Journal reveals the existence of more problems than it solves it will serve a useful purpose.

The objects with which the articles will be concerned will originate in one or other of the sciences or in science as a whole, and they will be examined in the light of wider philosophical considerations. This will give the Journal its distinctive character. Scientific problems as such are the concern of the various scientific societies, and are examined by them in their publications, but these societies do not—except very occasionally for some special reason—inquire into the relation of the problems to other realms of study. Similarly, the problems of general philosophy are the concern of philosophical societies and are examined in their publications, but such consideration as they give to science is incidental and is undertaken because the examination of science is found necessary to the study of philosophy and not because philosophy is necessary for the understanding of science. We do not, of course, in the least disparage either of these undertakings; on the contrary, we regard them as essential to a complete education.

They do, however, leave a conspicuous gap which we shall attempt to fill without encroaching on their peculiar preserves. We shall not publish articles which are narrowly scientific or which, on the other hand, seek to mould science to the demands of an independently conceived philosophy. Limits, of course, can never be precisely defined, and there will doubtless be borderline cases in which the exercise of discretion will be called for, but, in general, science, both past and present, will be taken as the primary datum for consideration and the consideration will be that of the philosophical implications contained in it.

Our appeal is addressed to all who are in sympathy with our aims, and are willing and able to assist in promoting them, but especially to the large and growing company of scientific workers with more than superficial knowledge of the theory and practice of modern science. We appeal to them to supplement the prosecution of their particular and limited tasks by reflection on the meaning of the great movement of which they form a part and by the study of its actual and possible effects on the future of both the individual and society. In their hands lies a power of which they are as yet imperfectly aware but which, consciously or unconsciously, they will use to shape the pattern of life in the days that lie ahead. Only by a determined effort to understand the powers and the limitations and the essential character of the Science of which, without such an effort, they will become the slaves and not the masters, will they succeed in turning their opportunity to good account.

Papers are invited from all qualified to write them. They will be carefully considered by the Committee of the Group, with whom will rest the responsibility for their acceptance or rejection. Correspondence also is invited, and authoritative reviews and essay-reviews of books on relevant subjects will be included in the Journal. An attempt will be made to keep readers in touch with studies in the philosophy of science in other countries : articles from distinguished foreign workers in this field will be published, translated into English if they are not originally written in that language. By this means it is hoped to remove some of the disadvantages arising from too restricted a range of viewpoints.

All correspondence should be addressed to the General Editor (Dr. A. C. Crombie, University College, Gower Street, London, W.C.1) who is also the Hon. Secretary of the Philosophy of Science Group of the British Society for the History of Science.

THE EDITORIAL BOARD

A THEORY OF MEASUREMENT

It is now generally agreed that the theory of relativity has made necessary a new conception of the nature of physical science. The change may be expressed briefly in this way. Formerly science was regarded as the study of an external world, independent of the observer whose experiments and observations were simply means of finding out how the world was constructed and by what laws its behaviour was governed. The emphasis has now shifted from the nature of the world to the operations of experiment and observation. These are no longer regarded as more or less arbitrary means of discovering the already established order of nature, but rather as affording primary data for rational study ; and any world that we may contemplate is no longer an independent existence whose nature demands or determines them, but rather a logical construct, formed and shaped and modified so as to afford a true picture of the relations which the observations exhibit.

This 'operational' view—a term first introduced, I believe, by Professor Bridgman of Harvard—is now generally accepted in so far as those departments of physics are concerned in which relativistic considerations are explicitly involved. Indeed, it is inescapable. It is admitted, for instance, that the mass of a body is a function of its velocity. It is agreed also that you may regard its velocity as anything you like from zero up to the velocity of light, absolute velocity no longer being granted any significance. It necessarily follows, therefore, that the mass of a body is anything at all from a certain minimum up to infinity, according to your choice. It does not become definite until you define precisely, in all essential details, what operation you are going to regard as determining it. Having defined that operation you must, of course, stick to it in all determinations of mass, but your original choice is unrestricted. Hence the mass of a body, once called the 'quantity of matter' in it and regarded as possibly ascertainable but essentially unchangeable, has become the result of an operation which can be varied in one detail at the will of the experimenter.

This, I say, is now generally accepted because it cannot be evaded, but the general implication, that the whole procedure of science

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[1] Chairman's Address at the Annual General Meeting of Philosophy of Science Group of the British Society for the History of Science, 7th March 1949

must be re-interpreted in conformity with this change of outlook, is very far from being accepted. The new view is adopted where it is unavoidable and avoided where its adoption is not immediately seen to be compulsory. We have in consequence a profound inconsistency running through science, and especially physics, which must be recognised and removed if a philosophy of science is to become possible. The purpose of this paper is to suggest how this can be done for a large part of the field of science—namely, that part which is concerned with measurement.

It might be thought that since the necessity for the new view was first realised in the field of metrical physics, and there seen to be so far-reaching as to affect all physical measurements in one way or another, there would be nothing left to do in this field, and one ought rather to seek to bring the non-metrical sciences into compatibility with a completely reformed physics. This, however, is very far from the truth. Surprising as it may seem, physicists thoroughly conversant with the ideas of relativity, and well able to perform the necessary operations and calculations which the theory of relativity demands, no sooner begin to write of the theory of measurement than they automatically relapse into the philosophical outlook of the nineteenth century and produce a system of thought wholly at variance with their practice. It would be invidious to select examples. Those who wish may choose from a library any book or paper on the theory of measurement written, say, since the first world war, and the probability that it will not exemplify the statement I have made is so small as to be almost negligible. It will be sufficient, perhaps, if I merely refer to the long series of papers on the theory of dimensions which appeared in the *Proceedings of the Physical Society* and the *Philosophical Magazine* during the recent war years. Let us glance briefly at the main features of the almost universally adopted principles on which current theories of measurement are based.

It is generally supposed that a measurement is the determination of the magnitude of some inherent property of a body. In order to discover this magnitude we first choose a sample of the property and call it a ' unit '. This choice is more or less arbitrary and is usually determined chiefly by considerations of convenience. The process of measurement then consists of finding out how many times the unit is contained in the object of measurement. I have, of course, omitted many details and provisos, for I am not criticising the thoroughness with which the matter has been treated but the fundamental ideas in terms of which the whole process is conceived and expressed. That being understood, the brief statement I have given will be accepted, I think, as a faithful account of the way in which the subject of measurement is almost invariably approached by those who seek to understand its basic principles.

Now it is obvious that this is in no sense an ' operational ' approach. ' Bodies ' are assumed, having ' properties ' which have ' magnitudes '. All that ' exists ', so to speak, before we begin to measure. Our measurement in each case is simply a determination of the magnitude in terms of our unit, and there is in principle no limit to the number of different ways in which we might make the determination. Each of them—each ' method of measurement ', as we call it—may be completely different from any other ; as operations they may have no resemblance to one another ; nevertheless they all determine the magnitude of the same property and, if correctly performed, must give the same result by necessity because they are all measurements of the same independent thing. Thus we find in our laboratory books nine or ten different ways of measuring the surface tension of the same boundary between two substances, and if they do not all give the same result when the unavoidable ' errors of measurement ' have been allowed for, we must have made some mistake in carrying them out. Over and above them all there is a sort of platonic ideal surface tension which by our various devices we try to discover.

I want to suggest that all this is completely incompatible with the view of physical science which the theory of relativity forces us to take. Instead of supposing a pre-existing ' property ' which our operation measures, we should begin with the operation and its result and then, if we wish to speak of a property (which I do not think we shall do), define it in terms of that. The result of each operation should initially have its own name, and if we find that different operations yield approximately the same result, then we have made the empirical discovery that the quantities represented by the different names are approximately equal. It may then be convenient to give them a common name, but it would be understood that that was merely for convenience and that it had an empirical and not a logical foundation which further experience might cause us to modify. From this point of view the results yielded by our ten methods of finding surface tension are not ten strivings after the same ideal but ten independent quantities which we have discovered to be approximately equal.

The fundamental justification for this change of approach is, of course, that it is forced upon us if we wish to give the simplest description of what we do, and not weave a fairy tale around it. It is a direct statement of fact that the result of each of our surface tension experiments is the result of performing a specific operation, but it is not a direct statement of fact that it is a measure of a property of a surface which would exist whether we measured it or not. If we wish to use such language we must be prepared to give our reasons for asserting the existence of such a property ; when we have done that we must explain how we can maintain our belief in it in view of our other belief that we can change the magnitude of the property by changing our minds as to whether the surface is moving or not ; and if we are fortunate enough to surmount this second hurdle we have then to explain why we must add all this apparently unnecessary embroidery to the simple statement of what we have done.

Of course, the answer of the non-philosophical scientist would be that he uses the language to which he is accustomed and which happens to conform to the everyday commonsense view of the world which everybody takes for every-day commonsense purposes. Furthermore, since it makes no difference whatever to his practice or to the conclusions which he draws from his science whether he regards himself as measuring an independent property of surface tension by different methods or performing different operations which happen to give the same result, there is no sufficient reason for giving up a form of expression to which everyone has long been accustomed and in terms of which he can think most clearly. And if it were true that it makes no difference to his practice or to the conclusions he draws from his science, then this answer would, I think, be a good one. Until recently this could be maintained with some success, but with the great extension of scientific achievement which has taken place during this century the position is changed, and it is now no longer true that the change of language is purely formal ; it has become a matter of basic importance.

Let me give one or two examples. Suppose we make a measurement—say, that which is usually described as the measurement of the length of a rod, AB. We obtain a certain result—say, 3. This means, according to the traditional view, that the length of the rod is three times the length of the standard unit rod with which it is compared. According to the operational view, it means that the result of performing a particular operation on the rod is 3. Now suppose we repeat the measurement the next day, and obtain the result, 4. On the operational view, what we have learnt is unambiguous. The length of the rod has changed, because ' the length of the rod ' is the name we give to the result of performing that particular operation, and this result has changed from 3 to 4. On the traditional view, however, we are in a dilemma, because we do not know which has changed, the rod measured or the standard unit ; a change in the length of either would give the observed result. Of course, in practice there would be no dispute ; the measurements of several other rods with the same standard, before and after the supposed change, would be compared, and if they all showed a proportionate change it would be decided that the standard had changed, whereas if the other rods gave the same values on both occasions, the change would be ascribed to the rod AB ; if neither of these results was obtained, then both AB and the standard would be held to have changed. If an objector pointed out that this only made the adopted explanation highly probable but not certain, he would be thought a quibbler, and the practical scientist would (until recently, quite properly) take no notice of him.

But with the wider scope of modern science he can no longer be ignored. Suppose, instead of the length of the rod AB, we take the distance of an extra-galactic nebula, N. Then we do, in effect, find that of two successive measurements, the second is the larger. This means, on the traditional view, that the ratio of the distance of the nebula to the length of a terrestrial standard rod is increasing. But is the nebula getting farther away or is the terrestrial rod shrinking ? Our earlier test now fails us. In the first place, we cannot decide which terrestrial rod we are talking about, because precise measurements show that our various standards are changing with respect to one another faster than any one of them is changing with respect to the distance of the nebula, so that the nebula may be receding with respect to one and approaching with respect to another. But ignore that : let us suppose that on some grounds or other we have made a particular choice of a terrestrial standard with respect to which the nebula is getting more distant. Then how do we know whether it is ' really ' getting more distant or the standard is ' really ' shrinking ? If we make the test by measuring the distances of other nebulae we must ascribe the change to the rod, whereas if we make it by measuring other ' rigid ' terrestrial objects we shall get no consistent result at all. We ought, therefore, to say that the probabilities favour the shrinking of the rod. Actually we do not ; we say the universe is expanding. But essentially the position is completely ambiguous.

Let us look at another aspect of the matter. On the earth we use various methods of finding the distance from a point C to a point D : consider, for simplicity, only two of them—the so-called ' direct ' method of laying measuring rods end to end to cover the distance, and the indirect method of ' triangulation ' by which we measure only a conveniently short distance directly and find the larger one by then measuring angles and making calculations. On the earth these two methods give identical results, after unavoidable ' experimental errors ' have been allowed for, and of course we explain this, as I have said, by regarding these two processes as alternative methods of measuring the same thing. On the operational view there are two different operations yielding distinct quantities, the ' distance ' and the ' remoteness ', let us say, of D from C, and our result tells us that, to a high degree of approximation, the distance is equal to the remoteness. Now let us extend this to the distant parts of the universe. Then we find in effect that the ' direct ' method and the triangulation method no longer give equal results. (Of course they cannot be applied in their simple forms, but processes which, according to the traditional view, are equivalent to them, show that this is what we must suppose.) On the view, then, that there is an actual distance which our operations are meant to discover—which, if either, gives the ' right ' distance, direct measurement or triangulation ? There is no possible way of knowing. Those who hold to the direct method must say that triangulation goes wrong because it employs euclidean geometry whereas space must be non-euclidean ; the correct geometry would bring the triangulation method into line with the other, and the geometry which is correct is then deduced so as to achieve just this result. Those who hold to triangulation, on the other hand, must say that space is pervaded by a field of force which distorts the measuring rods, and again the strength of this field of force is deduced so as to bring the two measurements into agreement. But both these statements are arbitrary. There is no independent test of the character of space, so that if there is a true distance of the nebula we cannot know what it is. On the operational view there is no ambiguity at all ; we have simply discovered that distance and remoteness are not exactly equal, but only approximately so, and then we proceed to express the relation between them.

Examples could be multiplied, but I think I have said enough to show that in the present state of science we can no longer be satisfied with a view of measurement which necessarily involves us in unresolvable dilemmas by arbitrarily inserting into our description things which must remain essentially inapprehensible. We must return to simplicity and describe what we do as faithfully as possible, without introducing conceptions which have outlived their usefulness. I therefore suggest the following definition : *A measurement is any precisely specified operation that yields a number.* This I hope to elucidate and in some respects amplify, but I do not wish at this stage to obscure the main point by introducing complications whose purpose cannot immediately be seen. Let us consider some aspects of this definition.

Perhaps the first point that will raise a question is its scope. There are an infinite number of possible operations that yield numbers, from counting raindrops to evaluating π, whereas the kinds of measurement that we actually make can be listed on a small piece of paper. Here the traditional view seems to be much nearer the mark ; we measure the properties of bodies, and since they have only a few properties we make only a few different kinds of measurement. But I think the restriction of our operations has quite another origin than this. Let us take an example. It would be possible to keep a standard copper ball, to let it fall from a given height on to various bodies, and to measure in each case the frequency of the note emitted on impact. This could be called, say, ' the sonority ' of the body, and it would be a true measurement according to my definition. And I see no fundamental reason why it should not be made. It seems *prima facie* as sensible as plunging a tube of mercury into one liquid after another and noting where the mercury comes to rest. Yet we do not measure sonorities and we do measure temperatures. Why ? The traditionalist would say : because bodies have temperatures but they have no sonorities ; but how he would know this I cannot imagine. I know of no revelation, human or divine, that declares what properties a body shall have, that allows specific heat but proscribes sonority, and that gives permission for viscosity if a liquid is moving slowly but withdraws it if the liquid hurries up. Nor do I know what body has what property corresponding to the measurement of the transit of a star across the meridian of Greenwich, say. This is undoubtedly a measurement, but why do we trouble to make it and yet ignore the time of transit of a cloud across the same meridian ? It has clearly nothing to do with properties, and we are interested in clouds as well as in stars.

I think the reason why we make some measurements and not others can be understood only in terms of the ultimate object of science, which is to find relations between the elements of our experience. If we passively accept experience as it comes the problem is far too difficult, so we establish artificial conditions that yield results more easily related with one another than natural occurrences are. Thus Galileo, in studying motion, did not begin with the movements of men and women and birds, of leaves driven before the wind, and so on, which provided his immediate experiences of motion, but instead made grooves in smooth inclined planes and rolled spherical balls along them ; he took his experience in the form of *experiment.* And secondly, out of the infinite number of experiments he might have made, he chose those which yielded *numbers*—i.e. he made *measurements*—because he could then use the comparatively well-developed machinery of mathematics to assist in finding relations of which he was in search. In the same way I picture the physicist as being perfectly free, within the limits of his capability, to select from the whole range of possibility of experience the particular examples which are most readily related to one another through his instrument of mathematics, which still far transcends any other branch of logical thought in scope and variety. That those examples are few is, I think, less an indication of our impotence in correlation than a consequence of the fact that a little goes a very long way, and that a few measurements, when their results are properly related together, make the performance of many others superfluous. It would be possible, for instance, to calculate our sonorities from data supplied by measurements which we actually make.

That this is a true account of the matter is indicated clearly enough by the history of physics. Consider temperature as an example. Here, to take only a few stages in the process, the standard of measurement has changed from the readings of a mercury-in-glass thermometer to those of a gas thermometer and then to those of a work thermometer. Why ? On the traditional view it is because we have successively got nearer to the ‘ true ’ scale of temperature. But how do we know that it is the ‘ true ’ scale ? Only because its readings stand in simple relations to other measures, particularly mechanical ones ; there is no other source of information. We must suppose, then, that our ‘ Nature ’ is essentially simple—a hypothesis which, it is true, has often been advanced but for which I can find no evidence beyond the fact that we give attention to her simple aspects and ignore the others. Why not, therefore, simply state the straightforward fact that we choose the measurements whose results are simply related to one another ?

If, then, we may accept the scope of our definition as simply a recognition of the freedom of the investigator to perform any operation he may find possible and useful, we may next turn to the phrase, ' precisely specified '. In what sense can we properly say that a measurement is a *precisely specified* operation ? Strictly speaking, no operation ever has been precisely specified in all its details and circumstances, nor can it be. In the first place, the specification would be unutterably tedious. If we take such a simple operation as that of measuring length, for example, we shall have to describe how to construct a standard measuring rod, including the testing of its material for various characteristics ; we shall then have to describe how copies are to be made ; and finally, how they are to be used in finding the length of the object we wish to measure. I doubt if this has ever been done. However, there is here no difficulty of principle. The process is well understood, and those engaged in it know very well when they are doing it properly and when they are not. It would therefore be possible, given sufficient time, to write out a full description of all essential details. But, secondly, what are ' essential ' details ? We say, for instance, that the thermometer shall stand at such and such a reading while the measurement is being made or, if not, then certain corrections to the result shall be made, but we do not say that the experimenter must have black hair and wear brown shoes, that there shall be a deep depression off Iceland, that Mr. Z. shall be Prime Minister, and that an infinite number of other circumstances, *which must necessarily take one form or another*, shall have such and such a form. What determines which details are essential ?

The answer is not, as we are at first inclined to think, that we specify all the details that are found to affect the result and ignore all the others. When we prescribe the process for measuring the position of a star, for instance, we do not say that it must be carried out at 5 h. 40 m. sidereal time, yet the result we get will depend very much on the time at which we make the operation. When we describe how a body must be weighed we do not say that the operation shall always be performed at St. Helena, yet the place of performance has a distinct influence on the result. The true determining factor, I think, is the same as that which decides what measurements we shall make—namely, that we specify only those conditions that are necessary to give us results that stand in simple relations to others. The position of a star, for instance, stands in a very simple relation to the time, and so we exclude the time from the conditions of measurement of it, and congratulate ourselves on having discovered a relation ; in our former, more picturesque, language, we have discovered that the earth rotates. I think this rule is perfectly general: the process of measurement is specified only in those details which are necessary to ensure that the results obtained are useful for our ultimate purpose of correlation. Too few would yield results too chaotic; too many would yield an excessive number of results too nearly identical.

But this is not all. There are two details that we not only leave unspecified but positively insist shall be left unspecified; it is a part of the specification, we may say, that they remain arbitrary. Every measurement may be divided into two parts—a manual part and a mental part—and one of these unspecified factors belongs to each part. Let me take them in turn.

In the manual part of a measurement the factor which we leave open is that to which, in the ordinary form of expression, we say the measurement refers. Thus, if we are stating how to measure length, we do not say that the rod AB must always be the rod measured; we still obtain a measurement of length if we substitute CD or EF or, in fact, anything else that will enable a similar process to be carried out. The operation for measuring length, then, must include, as a necessary part of its specification, the condition that one element of the process shall be variable in every possible way. The arbitrary part need not be a material body. If we are measuring the time of occurrence of an event, for instance, the event may be the collision of two bodies or the transit of a pure idea like the First Point of Aries. That is why we cannot describe this arbitrary part of the process more explicitly than as a 'factor' or 'element' of the operation.

This is, of course, little more than a re-wording of the familiar idea that a large number of bodies have the same kind of properties, such as length, mass, and so on, but it is a necessary re-wording because we no longer ascribe any significance to these 'properties'. Moreover, it emphasises the fact that the arbitrariness, although I have called it a 'necessary' part of the specification, is necessary only for practical reasons and is not imposed on us by the nature of experience itself. Thus there is no fundamental reason why we should not regard the measurement of the length of CD as a different operation from that of the length of AB, and call it by a different name—say, the 'extent'. But if we did so, the number of different kinds of measurement we would have to recognise would be so enormous that progress would be impossible. Instead, therefore, of speaking of 'length' and 'extent', we speak of the 'length of AB' and the 'length of CD', and the phrases must not be held to imply the existence of objects having the same property in different degree, but regarded simply as a convenient way of indicating how we have exercised our option in the choice of the unspecified part of the process.

The second unspecified factor occurs in the mental part of the operation, and it lies in the liberty which we reserve to ourselves to multiply the result of the operation by any constant number whatever. For example, to measure the length of a rod we take a scale constructed in the proper way, lay the rod along it (so much for the manual part), read the scale numbers at the two ends, and subtract the smaller from the larger (this is the mental part). We then reserve the right to multiply the answer by any number, n, provided, of course, that we do so for *all* length measurements which are to be brought into relation with this one. This, of course, is simply our way of expressing what is usually called ' changing the unit of measurement ', but not only is it a necessary re-expression, arising from our abandonment of the idea of sample properties called ' units ' ; it has also the great advantage that it makes the process definitely a *mental* one—it is a piece of arithmetic—whereas in current thought and practice the process is most indefinite. When we change the unit of length from the centimetre to the metre, for example, we simply multiply the results of our measurements by $\frac{1}{100}$, but when we change again from the metre to the yard, we change the *manual* part of the process, and change it in a way not covered by the permitted arbitrariness in that part. We are therefore actually making a different measurement, and that this is so in fact and not only in theory is shown by the circumstance that the standard metre and the standard yard are actually slowly changing in relative length. Yet we unthinkingly group together the change from centimetres to metres and the change from metres to yards as examples of the same purely conventional process of ' changing the unit of measurement '

When we seek for the reason for allowing this arbitrariness we find it again in the fact that it simplifies the subsequent correlation. If we did not allow it we would have to deal with very unwieldy numbers ; the range from the lengths of light waves to the distances of the nebulae is too great to be expressed conveniently on a single scale. If, on the other hand, we allowed ourselves to take the square root or the logarithm or any function of the result other than a simple multiple of it, we would no longer be able to maintain the principle of dimensional homogeneity which is of such use in expressing physical laws. It is very simple to prove that that principle depends on just this liberty and on nothing else.

I have no time to consider the question of dimensions at any length, nor is it necessary as I have done so elsewhere[1], and the theory of measurement which I am now describing is in fact the one to which the treatment of dimensions given there is naturally adapted. It will not be out of place, however, to point out how much more satisfactory the whole conception of dimensions appears from this point of view. Each measurement is a particular operation, and the dimensional symbol stands for that operation according to a simply stated convention. The liberty to multiply the result of any operation by any factor then necessarily demands the dimensional homogeneity of every physical relation between results of measurements. There is nothing mystical about the process ; it is a simple expression of what is actually done. Contrast that with the treatment of dimensions given in any text-book other than that of Professor Bridgman. You start with bodies ; the bodies have properties ; the properties have dimensions, which thus belong to the third order of metaphysical quantities. What are these dimensions ? No one knows—or, at least, no one has ever said ; they are simply some kind of hall-mark of the properties of bodies—or sometimes of units, the point seems unsettled. When you measure the length of a body you of course compare it with a unit length, and the result has the dimensions of a length. This is held to be true whatever unit you compare it with, but no reason is given for this assumption. Moreover, if you find the rod AB is three times as long as a rod CD, then the number 3 has no dimensions because it is simply a comparison of two lengths. If, however, you now decide to make CD a ' unit ' of length, then the ratio suddenly takes on the dimensions of a ' length '. Why ? No one knows. There are other mysteries also. The dimensions of some quantities—electro-magnetic ones in particular—are essentially mysterious. The quantities must have dimensions, but we cannot find out what they are. Again, it is felt by many that it would be disrespectful to allow fractional indices to occur in a dimensional symbol, though negative indices are permitted. All this mythology—which, it must be noted, cannot be dispensed with if we wish to use the principle of dimensional homogeneity and also to maintain our belief in the objective existence of ' physical properties '—is entirely swept away when we describe what we are doing literally and not by parable.

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[1] *Philosophical Magazine*, 1942, **33**, 321, and subsequent papers

Our precisely specified operation, then, is to be understood in this sense, that only those conditions are to be prescribed that are found by experience to be necessary to give results that stand in simple relations with other results, and we must always allow the two elements of the process which I have mentioned to remain arbitrary. I should add a word about another element of the process which, in fact, always remains arbitrary, though not as a necessary condition of the measurement—namely, the time at which the measurement is to be made. We do not, I believe, say of any kind of measurement that it must be performed at such and such a time, and call the same procedure, if performed at another time, a measurement of a different kind. This has sometimes been held to be a necessary element of measurement, but I can see no grounds for such a view. I think that at bottom the reason why we never prescribe the time is the same as the reason why we never prescribe the size of the operator's boots—namely, that nothing would be gained, and much complication would be introduced, by doing so. Moreover, as we shall see, there is great advantage in reserving the liberty of seeing how our measurements are related to time ; this would be impossible if the time of performance was part of the specification of the measurement. It should be noted that, contrary to what is sometimes said, we do not so universally ignore the *place* of operation. The determination of the sidereal time of an event, for instance, is an important measurement, and it must be performed at a particular place. If it is performed somewhere else it is a different measurement—in other words, sidereal time is always local—and the relation between its result and that of the first measurement is an important quantity which has been correlated with the shape of the earth. The reason why this difference of treatment of time and place is convenient is related to the fact that, with respect to any system of co-ordinates at all, it is possible to be in the same place at two times but not at the same time in two places—a simple fact of experience which should be sufficient to refute the commonly heard statement that time and space are simply conventionally separated parts of an objectively existing homogeneous ' space-time '.[1]

I therefore believe that the time at which a measurement is to be made is left unspecified not for such fundamental reasons as those which lead to the two kinds of arbitrariness which I have mentioned, but mainly because it enables us to ' repeat ' our measurements. This question of repetition is of great importance, and I want to give some attention to it. Consider a particular measurement—say a measurement of length or mass or temperature or what you will. We carry it out with a particular choice of our two arbitrary factors, i.e. in the normal case, on a particular body, A, adopting a particular ' unit of measurement '. We get a certain result. We then repeat the process several times in succession, still with the same choice of the arbitrary factors. The results are rarely, if ever, exactly the same, but the variations may be of two kinds. If we plot the measures against the times of performance of the operations we may find that the readings show a systematic change, or variations which we can only describe as random. In each case we draw a smooth curve to which the various points approximate as closely as possible: in the first case it may be any simple kind of curve ; in the second case it is a straight line parallel to the time axis. We then take, as the ' true ' measure at any instant, the point on the curve corresponding to that instant, and not the result actually obtained. This means that we never accept the result of a single operation, but only a certain function of the results of a number of operations. We can now amplify the phrase, ' strictly specified operation ', in our definition of a measurement, by saying that the operation must be divisible into several parts, identical except for the time of performance, and must be succeeded by a process which we may call ' averaging ' the numbers obtained in the several parts.

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[1] For a more detailed examination of this idea see *Proc. Aristotelian Soc.*, 1948, **48**, 153.

How many parts must there be ? In other words, how many times must we repeat our determination ? The answer can only be : as many times as are necessary to indicate a unique curve. Unfortunately, however, this appears to mean an infinite number of times. If we compare the length of the Imperial Standard Yard with that of one of its copies say 100 times within six months, we shall apparently find ample evidence of a straight line parallel to the time axis. If we go on repeating the process at the same rate for 60 years we shall find that the best curve is no longer a straight line parallel to the time axis. If we determine the duration of the sidereal day every day for a year by the best quartz crystal clocks obtainable, we shall again find a straight line parallel to the time axis, though the individual points may show considerable scatter. There is reason to believe, however, that if we continued the habit for 1,000 years, we should find that the end of the line had moved $\frac{1}{100}$th second away from the time axis. It becomes more and more evident as the precision of our operations improves, that no finite number of repetitions of a process is sufficient to give an average which will not require to be changed when further repetitions have been made. This, it must be noted, does not mean merely that the result of a measurement is a function of time and may be different in a thousand years' time from what it is now. It means that we do not know what it is now, for a change of curve means not only that the continuation of the present curve will be different from what we expected, but that the present curve itself is not the true curve ; what we thought was a straight line is perhaps a part of a parabola, which can have at most two points in common with the line.

From the older point of view the implication of all this is that we can never determine the magnitudes of our physical quantities with certainty : the nature of the physical world must remain for ever unknown. From our point of view, from which the results of our voluntarily chosen operations are seen as starting points and a complete system of rational relations between them as the goal, it means that if we define our operations so that they include only a finite number of repetitions, we must expect that the relations we find between their results will not be final ones. This we learn from experience. We are then faced with the problem of prescribing the operations so that they yield results whose relations are as comprehensive and lasting as possible, and it is an obvious addition to our specification to say that the number of repetitions must be at least sufficient to leave us in no doubt about the simplest smooth curve to be drawn. Experience shows that this is both definite and practicable, and if in the course of time we find that a better choice might have been made, we can either improve the specification and simplify the relations, or retain the previous specification and be satisfied with comparatively complex relations. We are perfectly free to choose which to do, and we are not haunted by the thought that we are doomed for ever to pursue the essentially unknowable. At each stage our knowledge, such as it is, is definite and incontrovertible. That operations defined in such and such a way yield results that are related in such and such a way is not subject to alteration as the consequence of further knowledge. What further knowledge can do is to show that if we substitute operations performed in a slightly different way, we shall obtain simpler and more comprehensive relations between their results.

I believe this feature of our definition—that repetitions are an essential part of the specification of a measurement—is profoundly important for the proper understanding of what we speak of as 'probability', so I want to contrast it as clearly as possible with the traditional view. When we start with the assumption of a world of physical objects having measurable properties, each single performance of a so-called operation of measurement is a self-contained determination of the magnitude we are seeking. If we could perform it perfectly, no repetition would be necessary for the magnitudes regarded as constant, and none would be possible for those regarded as changing. The purpose of repetition is solely, in the first case, to eliminate the effect of human imperfections or uncontrollable disturbing influences which prevent the process of measurement being carried out with strict accuracy. It is assumed that since we have no means of removing or rigorously controlling these imperfections and disturbances, they will altruistically take upon themselves the duty of cancelling one another out when the average of many determinations is taken. This seems to be borne out by experience because the average of several determinations usually repeats itself more accurately than a single determination does, but why this should be so is essentially mysterious. Since the disturbing influences can, by hypothesis, work independently and in any direction, why should they behave as though they were constrained to exhibit a strict equality of occurrence on the average ? The question has often been posed but never answered, and I believe it is unanswerable because the whole way of looking at the matter to which it belongs is essentially wrong. If our postulation of entities goes beyond the inescapable demands of experience we must expect to have to make assumptions beyond the requirements of reason in order to square them with experience.

But now look at the matter from the operational point of view. Here we make no preliminary assumptions about quantities having magnitudes which we seek to discover. As pure seekers we perform operations of various kinds until we find some whose results stand in simple relations with one another. We find that the only operations that give such results are those which include several repetitions of a process and a subsequent averaging of the numbers which the process yields. We therefore restrict ourselves to such operations, regarding the repetition and averaging as an essential part of a measurement.

Now let us consider, from these two points of view, one of the simplest and most commonly discussed problems in probability, namely, the result of tossing a coin *n* times. This operation is not usually regarded as a measurement because, according to the older view, what we obtain from it—the ratio, let us say, of the number of heads to the total number of throws—is not readily regarded as a property of anything in the external world. From our point of view, however, it is in no respect different from a recognised measurement such as the measurement of a length ; it is an operation, prescribed in all essential details, which yields a number, and we find by experience that when we repeat the tossing say 100 times, and perform a mental operation on the result analogous to averaging, we obtain a magnitude which does stand in a simple relation to other magnitudes. It is approximately equal, for instance, to the magnitude obtained from the next performance of the complete process, and I have no doubt, though I do not know of any experiment on the subject, that there would be a relation between it and the position of the centre of gravity of the coin with respect to its centre of figure. It is true that this particular measurement does not exhibit sufficiently extensive correlations with other measurements to be of importance in physics. In that respect it is something like our ' sonority ', and so it is not customarily performed in physical laboratories, but that is irrelevant, of course, to our present considerations. It is essentially a measurement, in no respect different in its own character from any other measurement, and the only reason against its regular performance is that its results do not help us sufficiently in our endeavour to construct a far-reaching system of correlations.

We are thus freed from the sense of bewilderment which I suppose everyone has felt at one time or another at the curious fact that although each individual toss may give heads or tails, irrespective of the result of any other toss, a group of 100 or more with a given coin nevertheless gives a percentage of heads that repeats itself with only small deviations. How can the individual throws be independent of one another when collectively they show a strong correlation ? I think that this fact must remain inexplicable so long as a physical relation is taken as an outward and visible sign of some connecting link between objective entities in an external world. But when the process of tossing a coin is regarded simply as an operation in which some conditions are prescribed and others not, we see that the individual toss is analogous to the single determination of a length ; it may give any result within a certain finite range, but the average of many determinations is an approximately constant quantity. It is true that the range is much smaller in the second case than in the first, but that is a detail ; it simply means that the conditions of the experiment which are not prescribed have less influence in the length determination than in the tossing. But essentially the scatter of the individual determinations of a length is of the same character as that of individual tosses of a coin. We assign it rather vaguely to ' personal equation ' or something of that kind, and without examining the fluctuations we do our best to eliminate their effects by ignoring the individual results and considering only the average. Since physics is not the whole of science, that is legitimate enough in a purely physical investigation, but as complete scientists we must not forget that there is a problem left over for the psychologist, and its unavoidability shows that ultimately physics and psychology are not entirely independent sciences and that a complete understanding of what we ordinarily call the material world must involve them both.

From this point of view, then, there is nothing peculiar about the fact that the average of 100 tosses is an approximately constant quantity. It remains mysterious in the sense that we could not have predicted it without trial—i.e. that we can see no *a priori* reason why it should be so. But equally there is no *a priori* reason why the lengths of bodies should remain approximately constant ; we know that only from experience, but the experience is such a common one that we tend to accept it as natural. The coin-tossing result then stands out as a special sort of mystery. This, I think, arises entirely from our false view of what we are doing. If it is once recognised that all possible operations are open to us, that some give regular results and some do not, and that ultimately we can determine which is which only by trial, then all the special mystery that seems to characterise statistical results disappears, and we are left with only one perfectly general mystery, namely, why experience is what it is and not something else. And that is not a logical problem, but an elementary emotion.

I have dealt with only one trivial example, for want of time, but I think that when the whole theory of probability is re-examined from the operational point of view it will be found to be greatly clarified. The mathematical calculus, of course, will be unaffected, but the view of what the calculus represents in terms of physical conceptions will be transformed, and since it is this view that is the mainspring of all progress in the subject, we may expect the change to have physical as well as philosophical consequences. I have time for only one more example of an important effect of the whole-hearted adoption of the operational view of measurement, and I will take the fundamental theorem of quantum mechanics, expressed metrically in Heisenberg's uncertainty principle and in a more general philosophical form in Bohr's principle of complementarity.

You will remember that the idea underlying Bohr's principle is this. When we make a measurement—or an observation, as, in more general terms, he calls it—we are concerned with two things, the thing to be observed and the instruments of observation. We have some freedom in dividing the whole system before us into these two parts ; for instance, if we are looking through a microscope focused on a particle we may make the particle the thing to be observed and the eye and microscope instruments of observation, or we may make the microscope and particle the system observed and the eye the instrument of observation. But somewhere or other the division must necessarily be made. We are now faced with a dilemma. If we are to define precisely where we make it—i.e. to define in all essential details what is the thing we are measuring as distinct from the means of measuring it—we are precluded from measuring at all, because we cannot do so without allowing an interaction between the thing to be measured and the instruments, which necessarily alter the thing we have defined. The condition of validity of our definition of an objective system is therefore the renunciation of all claim to know anything about it. If, on the other hand, we are not prepared to make this renunciation but insist on observing our system, then we can do so, but we automatically lose the knowledge of what system it is that we have observed. From this dilemma, as Bohr points out, there is no escape. We must choose between a sharply defined object of measurement, of which we are precluded from making the observations necessary to describe it as a space-time system, and a space-time description of a system which, because of its transmutation when it is observed, necessarily fails to exhibit a behaviour which can be described in terms of causes and effects.

Now it will be seen that the validity of this argument depends entirely on the original postulate that the whole collection of things with which we are concerned must be divided into a thing to be measured and a means of measurement. This is, in fact, the traditional view of measurement which I hope I have shown must be abandoned on grounds quite independent of the quantum theory, because it leaves us in a perpetual state of uncertainty about what any particular measurement means. We perform it, get a definite result, and then proceed to make the result indefinite by voluntarily regarding it as a ratio of two entities which must remain eternally unknown. When we cease to add this unnecessary voluntary act the ambiguity goes, and with it the whole foundation on which the principle of complementarity rests. The equivalent, on the operational view, of the division into 'thing to be measured' and 'means of measurement' is the stipulation that one element of the manual part of the operation must remain arbitrary, and that is a requirement that can be met without ambiguity. When, for instance, in prescribing the operation for measuring length we say : 'Lay the object AB along the scale . . .', and then add, 'The object AB may be anything with which this operation can be performed', we are stating something perfectly unambiguous, and when we call the result 'the length of Mr. X's pencil' we simply mean that it is the result of the measurement of a length when Mr. X's pencil is the object AB. By defining precisely, in a particular case, what the object AB shall be, we do not preclude ourselves from carrying out the operation or getting a definite result, and we are perfectly free from ambiguity so long as we do not try to make that result a metaphysical property of a self-existent pencil.

The examples which are always given to illustrate the working of the uncertainty principle offer no difficulty. They are always concerned with observations of electrons or similar particles, and when we translate them into operational terms we see immediately that they are illegitimate. You can perform the operation on Mr. X's pencil, but if you say : 'Let AB be an electron', then you cannot perform it. This, of course, is a simple fact, and if anyone is inclined to question it I would ask him to perform the operation of measuring the diameter of an electron at a time and place of his own choosing, and I would be delighted to witness the achievement. It is regarded as a legitimate example because we first adopt the older view of measurement as a determination of a property of an independently existing object, and, secondly, suppose that an electron is an example of such an object. But even if we allow the first premiss, the second fails because, as a matter of historical fact, the electron has not been observed but conceived in order to express relations between things which are observed, and it is therefore not the same kind of entity as an observed body. If we do not allow the first premiss, but instead regard measurements, defined as the results of operations, as our primary data, electrons are still more obviously elements of the logical system which we construct in order to express the relations found between measurements ; they are most emphatically not things with which manual operations can be performed.

The question how far our knowledge is certain and how far essentially uncertain now takes on a different aspect. Since we no longer admit a pre-established order of things which we seek to discover, we cannot speak in the ordinary sense of the ' exactness ' with which we can determine it or the ' certainty ' that our conception of it may be the ' true ' one. Modern physics, inconsistently adopting this presupposition, has decided that we can never determine it exactly or with certainty. The equivalent, in our terms, of the question involved is this : is any system of relations which we find between the results of our measurements established finally or is it subject to change when further measurements are made ? The answer is clearly, I think, that we can never be certain that no change will be required. This follows first of all from the fact that no amount of past experience can remove the possibility that future experience may be something other than we expect. So long as we can imagine alternatives to the occurrences we predict, we can never feel absolutely certain that they will not happen. And, secondly, it follows, as we have seen, from the fact that, for practical reasons, we are limited to a finite number of repetitions of our experiments and we can never be sure that additional ones would not change our results.

We can thus accept the implication of the quantum theory with regard to the essential uncertainty of our predictions, but without the accompanying dilemma regarding the description of the knowledge we already have. That dilemma arises from a view of measurement that is no longer tenable, and it may, indeed, be itself regarded as a *reductio ad absurdum* of that view. But of course the quantum theory is not thereby discredited ; it is the *interpretation* of it, the philosophical consequences that it has been held to entail, that are seen to be unsound. The equations of the quantum theory and the importance of the magnitude known as Planck's Constant remain unaffected. The whole of the criticism I have been making is concerned with the nature of measurement itself, irrespective of the particular results it is found to yield when actually applied. Planck's Constant, on the other hand, arises from the relations which we in fact find between the numbers obtained when certain measurements are performed. Nothing that I have said has any bearing on that. But it is at least as important to understand what our results mean as to achieve those results, and it is, in fact, for just that purpose that this Group has been formed.

We are now at a stage when old conceptions in physics have broken down and new ones have begun to take their places. Such a period, like the time of change from the medieval to the modern view of the universe, is full of exhilaration, but it has its dangers. The older views, within their limited range, were consistent, and the newer ones, within a wider range, may be consistent also. But in the time of transition from one to the other we are prone to entertain both at once, without being aware of the contradiction thus introduced into our thinking. Just as Copernicus, though he had destroyed all grounds for belief in the celestial spheres, nevertheless kept them in his new picture of the universe, so we, having destroyed all grounds for belief in objective measurable properties of bodies, still keep them in our theories of measurement and dimensions and in our interpretation of our latest discoveries. The object of this paper is to point out the need for a complete reform and to indicate some of the directions in which it can be made.

HERBERT DINGLE

CRITICAL EPOCHS IN THE DEVELOPMENT OF THE THEORY OF SCIENCE

1 *Introduction*

The present paper is born from the conviction that recent discussions on the foundations of mathematics and physical science cannot be fully understood without reference to their historical and philosophical background. These discussions for the greater part originate not merely from the results of contemporary scientific research in themselves, but rather from the incompatibility of these results with certain preconceived philosophical doctrines.

This fact is, indeed, not altogether ignored by men of science, but nevertheless they seldom go farther back in the history of thought than to Kant's criticism, the importance of which in this connection is thereby greatly exaggerated. In my opinion, we must go back at least to Aristotle if we want to grasp the roots of the doctrinal divergences to which the results of modern research into the foundations of mathematics and physical science have given rise.

As a matter of fact, it was Aristotle who, inspired by Plato's philosophy, systematically set forth a theory of science, which for centuries to come directed and even dominated scientific and philosophical thought. We will see that the differences of opinion with regard to the foundations of science have for the greater part sprung from the inconsistency of certain fundamental results of modern research with the main theses of the Aristotelian theory of science, which turns out to be incapable of dealing in a satisfactory manner with the problems originating from contemporary foundational research.

2 *Aristotle's Theory of Science*

The essentials of Aristotle's theory of science[1] may be compressed into the following definition of ' deductive ' or, as Aristotle calls it, ' apodeictic ' ' science ' :

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[1] For a full account, see H. Scholz, ' Die Axiomatik der Alten ', *Blätter für deutsche Philosophie*, 1930–31, *4* ; cf. W. D. Ross, *Aristotle*, 4th ed., London, 1945, p. 43. Most relevant texts are found in I. M. Bochenski, *Elementa Logicae Graecae*, Roma, 1937. For critical discussions : F. Enriques, *Problemi della Scienza*, Bologna, 1906 ; J. Dewey, *Essays in Experimental Logic*, Chicago, 1916 ; F. Goblot, *Traité de Logique*, Paris, 1920 ; B. Schultzer, *Transcendence and the Logical Difficulties of Transcendence*, Copenhagen-London, 1935

A deductive science is a system S of sentences, which satisfies the following postulates:

(1) Any sentence belonging to S must refer to a specific domain of real entities.[1]

(2) Any sentence belonging to S must be true.

(3) If certain sentences belong to S, any logical consequence of these sentences must belong to S.

(4) There are in S a finite number of terms, such that
  (*a*) the meaning of these terms is so obvious as to require no further explanation;
  (*b*) any other term, occurring in S, may be defined by means of these terms.

(5) There are in S a finite number of sentences, such that
  (*a*) the truth of these sentences is so obvious as to require no further proof;
  (*b*) the truth of any other sentence, belonging to S, may be established by logical inference starting from these sentences.

The postulates (4*a*) and (5*a*) constitute the so-called *evidence* postulate.[2] The fundamental terms and sentences, referred to in postulates (4) and (5), are called *principles* of the science under consideration. The postulates (1), (2) and (3) will be called respectively the *reality*, the *truth* and the *deductivity* postulates.

3 *Opposition from the School of Megara*

Aristotle's doctrines met with a fierce and systematic opposition from the school of Megara, the founder of which, Eucleides of Megara, though himself a disciple of Socrates, already had been engaged in dispute with Plato.

We can say for certain that Eubulides attacked Aristotle's truth postulate by stating the liar paradox,[3] and his evidence postulate by citing the so-called 'larvatus' sophism,[4] whereas Diodorus Cronus argued against Aristotle's acceptance of the reality of movement and against his modal logic. From the facts that Aristotle rejected the doctrine of the quantification of the predicate and that Stilpo, a somewhat later adherent to the school of Megara, required the identity of subject and predicate, we may infer that this doctrine was defended by the school of Megara.

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[1] *Analytica Posteriora I*, 28, 87a 38; cf. *I*, 7, 75a 38

[2] *Analytica Posteriora I*, 2, 72a 27; *I*, 3, 72b 5; cf. *Metaphysics Γ*, 4, 1006a 5; Plato, *Phaedo*, 107B

[3] 'If I say: "I am lying", do I lie, or do I speak the truth?' 'You are lying!' 'But if I lie, my statement was in agreement with the facts, so I spoke the truth.' 'So you were speaking the truth!' 'But if I spoke the truth, my statement was contrary to the facts, so I was lying.' Cf. A. Rüstow, *Der Lügner*, Leipzig, 1910.

[4] 'Do you know your father?' 'I do!' 'How is that possible? If I show

Nevertheless, these objections, though in Antiquity taken quite seriously (later authors, such as Prantl and Zeller, abusively dismiss them as trivial fallacies), could not prevent the triumph of Attic philosophy.

4 *Traditional Metaphysics as a sequel to Aristotle's Theory of Science*

Aristotle's theory of science of necessity demands a metaphysics as a science of the principles.[1]

The special sciences turn out to be incapable of giving a foundation for their own specific principles without lapsing either into a vicious circle in definition or in proof, or into an infinite regress. Nevertheless, it appears to be desirable to subject these principles, which special sciences have to adopt without definition or proof, to a close inspection. This is the specific task of metaphysics, which therefore may claim to provide scientific knowledge of the highest order. Consequently, Aristotle's metaphysics is what is now commonly called *foundational research*[2]; the designation as ' first philosophy ' seems quite apposite indeed.

In his foundational research, Aristotle deals especially with the principles of physics ; this accounts for its traditional designation as ' metaphysics '. The principles of mathematics are only incidentally considered. For instance, Aristotle attempts to prove, against the followers of Pythagoras as well as his own master, Plato, that it is not possible to found physics upon the principles of mathematics.

Metaphysics in the sense of Aristotle is therefore specifically an enquiry into the principles of physics and, more generally, any

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you a man completely wrapped-up, and if I ask : " Do you know him? "—what will be your answer? ' 'That I don't know him, of course! ' ' But suppose he happens to be your father ; then, if you don't know that man, it is clear, that you do not know your father.' Sextus Empiricus, *Adversus mathematicos*, VIII, 10. The relation of these arguments to later developments of the theory of science will be discussed in a second paper to be published in this Journal.

[1] *Analytica Posteriora I*, 9, 76a 16

[2] *Metaphysics*, K, 4, 1061b 2

research into the principles of science. Accordingly, in this more general sense Kant could speak of ' metaphysical principles of physical science ', Gauss of ' metaphysics of mathematics ', Cournot of ' metaphysics of infinitesimal calculus '.

In recent times, the term ' metaphysics ' is often used in a somewhat different manner, namely, as a designation for a theory of the suprasensible. This terminology is, in fact, closely connected with Aristotle's, who, as well as Plato, locates the principles of the sensible in the suprasensible.

5 *Traditional Theory of Knowledge as a sequel to Aristotle's Theory of Science*

Theory of knowledge, as a theory about the origins of human knowledge, also takes its *raison d'être* from Aristotle's theory of science.

If all scientific knowledge is acquired by means of logical inference starting from a certain number of immediate, irreducible principles, inevitably the question arises : whence do we obtain these principles ; in what manner may we account for our possessing and using them ?

This question has been dealt with already by Aristotle himself.[1] In his opinion, we obtain the principles by way of an intuitive vision. This intuitive vision of the principles originates from induction on the basis of sense perception. This doctrine has a curious theologico-anthropological background which should be mentioned here, as it has consequences important for the practice of scientific research.

Aristotle is convinced that perceptible phenomena are manifestations of World Reason, or νοῦς, to which human mind is akin. Therefore man is able, starting from sense perception, to grasp the deepest causes behind phenomena ; this is *induction*, which affords us an intuitive vision of principles.

According to this doctrine, the principles of science need not be detected by way of intentional systematic research ; a summary inspection of perceptible phenomena is sufficient. Knowledge of the principles is no monopoly of the philosopher ; it is proper to any man. All that philosophers have to do is to test and sift current opinions regarding the principles. This accounts for the important rôle which doxography, the description of the different opinions on a given subject, has always played in peripatetic science. As Goethe said, 'Alles Gescheite ist schon einmal gedacht worden; man braucht nur zu versuchen, es noch einmal zu denken'.

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[1] *Analytica Posteriora II*, 19, 100b 5 : cf. H. Maier, *Die Syllogistik des Aristoteles*, Tübingen, 1896, I, p. 22, n. 1 ; W. Windelband-A. Goedeckemeyer, *Geschichte der abendländischen Philosophie im Altertum*, 4. Aufl., München, 1923, p. 180

6 *Analogy of Aristotle's Theory of Science to Mystical Doctrines*

There is a striking analogy between Aristotle's theory of science and certain mystical doctrines.

As a matter of fact, Aristotle's metaphysics aims at administering to man an initiation which is an intellectual counterpart of the initiations he received in ancient mystery cults. This initiation is intended to liberate him from the continuous wonder caused by his inconstant destiny, by giving him once for all an explanation, definitive and irrevocable, of his experience. Such an explanation is implied in the knowledge of the principles; metaphysics as the science of principles therefore affords us the supreme wisdom.

By this knowledge of the principles, man is initiated into the mysteries of life, and he acquires intellectual immortality; he becomes inaccessible to wonder, which is considered as intellectual death. Therefore, according to Plato and Aristotle, man must be thrown into perplexity in order to become capable of the intuitive vision of the principles, just as in ancient mysteries he must suffer ritual death in order to rise to eternal life.[1] Consequently, a metaphysician is a man who has been initiated into the mysteries of life and who has found the clue to the enigma of life.

The ancients fully realised the analogy between metaphysics and mysticism; this appears from a text by Psellus, which has been published by J. Bidez and which is entirely inspired by Aristotle's doctrines.[2] I restrict myself to quoting a few essential phrases.

> . . . if it is by means of proof or dialectical analysis that you want to learn something, I am ready to teach you; but if you intend to approach your spirit to the intelligible by way of immediate contemplations of the spirit, I am not willing to instruct you. . . . Indeed, these are the two elements which are present in any literature, that which is inspired by God as well as the remaining part, namely, profane literature: the didactical element and the initiatory element. The first, however, comes to men by way of hearing, the second if the spirit has experienced

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[1] *Metaphysics* A, 2, 982b 12; *Ethica Nicomachia* X, 7, 1177b 25; Plato, *Theaetetus*, 155D, *Republic*, 534B, C.

[2] I take the passage from J. Croissant, *Aristote et les Mystères*, Liège and Paris, 1932.

> illuminations. This last has been called by Aristotle ' of the type of the mysteries ' and resembling the Eleusines ; for in those feasts, the initiated receive spectacles of impressions, but no instruction. On the other hand, he who by way of a personal movement of the spirit has acquired the immortality of the soul, has obtained didactical, but no initiatory, teaching ; but if anybody, thanks to a vision of the very eyes of the spirit, has seen the soul itself, or without contemplating the soul, has immediately received immortality, this one has experienced impressions and is initiated.

We learn from this text that indeed philosophy, as well as the mysteries, claimed to confer immortality. Therefore, according to Plutarch,[1] ' Plato and Aristotle call *epoptic* that part of philosophy in which we exalt ourselves to the first principle ', namely, metaphysics. This claim well explains the peculiar character of metaphysics, especially its attempts at giving definitive and irrevocable explanations, in accordance with the evidence postulate.

7. *Perennial Influence of Aristotle's Theory of Science*

Aristotle's theory has until quite recently directed scientific research. It was indeed accepted with such unanimity, that nobody even thought of imputing to Aristotle a special merit for establishing it or of devoting study to its origin, development, and further destiny.

The acceptance of Aristotle's theory of science naturally implied the adoption of the problematics of his metaphysics and theory of knowledge. Consequently, Aristotle's theory of knowledge as well as the problematics of his metaphysics and theory of knowledge are traditional elements in any system of speculative philosophy.

The answers given to the fundamental problems of metaphysics and theory of knowledge, to the questions as to what are the principles of the different sciences and what is their origin, however, greatly diverge. Aristotle's theory of induction, especially, has in recent times been superseded by other, partly strongly dissentient, views. Nevertheless, his doctrine, according to which science has to start from a certain number of fundamental notions and fundamental theses which no result of scientific enquiry ever can refute or even modify or restrict as to its field of application, has always been generally accepted, notwithstanding the divergence of opinions as to the actual enumeration of the principles and the explanation of their origin. In this connection, Descartes' ' innate ideas ', Leibniz' ' primae veritates ', Kant's

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[1] Plutarch, *De Iside et Osiride*, 77

'synthetic judgments *a priori*', should be mentioned. Usually, the fundamental notions and theses of logic and mathematics, as well as the notion and law of causality, are included among the principles.

8 *Importance of Mathematics for Traditional Philosophy*

The reasons for the importance of mathematics in relation to the fundamental conceptions underlying traditional speculative philosophy can now be stated without difficulty.

First of all, mathematics constitutes the classical example—practically the only one which is generally accepted as such—of a deductive science in the sense of Aristotle's theory. Aristotle himself takes his illustrations mainly from the mathematical sciences.[1]

In this connection it might cause wonder that he chose physical science, not mathematics, as the subject of his own foundational research. The historical situation, however, affords an entirely convincing explanation of his preference. Mathematics had recently been subjected to a thorough foundational research by the mathematicians Eudoxus and Theaetetus, who were in close relations with his master, Plato ; the Stagirite was completely familiar with their results, with which he entirely agreed. Therefore, in this domain there remained hardly anything to do. On the other hand, the foundations of physical science remained highly dubitable, as Aristotle convincingly shows in the first book of his *Metaphysics*. According to Parmenides and Plato, a deductive science concerning motion and change was completely inconceivable.

In his researches into the foundations of science, Aristotle consciously took Eudoxus's and Theaetetus's work as an example. His idea of metaphysics as a general ontology was inspired by their theory of proportions, which he used to refer to as 'universal mathematics'[2]. Here we have a second reason for the importance of mathematics for traditional speculative metaphysics.

There is no real foundation for the wide-spread opinion[3] that Aristotle was a poor mathematician. All we may infer from his manner of choosing mathematical examples is that his interest was focused upon the foundations of mathematics, which seems to me quite natural in a philosopher.

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[1] T. Heath, *Mathematics in Aristotle*, Oxford, 1949

[2] *Metaphysics* E, 1, 1026a 25

[3] This opinion is even found in T. Heath, *l.c.*, p. 1

9 *Revision of Aristotle's Theory of Science*

Aristotle's theory of science postulates, as we have seen, any science to be deductive, to start from obvious principles, and to have an empirical foundation. About 1600, it became more and more clear from scientific practice that sciences could hardly fulfil these three postulates at once.

By mentioning the date 1600, I do not want to imply that only then the development of modern, non-aristotelian, science took its start. It is well-known from the studies of scholars such as P. Duhem, E. J. Dijksterhuis, P. Rucker, A. C. Crombie and others that the roots of modern science reach back far into the Middle Ages, and there may be much truth even in the opinions of authors such as R. Eisler and A. M. Frenkian[1], who place the origins of a certain number of fundamental conceptions of modern science and philosophy in times long before Greek Antiquity.

It was not until 1600, however, that non- and anti-aristotelian conceptions took scientific forms which could successfully rival and even supersede the solid edifice of peripatetic science. From then onward, it became customary to recognise two different types of science, one of which conforms to the postulates of deductiveness and evidence, whereas the other answers to the requirement of an empirical foundation. Rationalism, as defended by Descartes, has a preference for the first type of science, which we will call *rational* science, whereas empiricism, typically represented by Locke, fosters *empirical* science, by which we mean the second type. The opposition between the schools of rationalism and empiricism, however, should not be overrated, as these schools derive their origin from the same historical situation and their doctrines show quite a number of common features. We shall see that a man like Nieuwentyt succeeds in reconciling the main theses of rationalism and empiricism ; the same can be said of Leibniz. And the expansion of modern scientific thought is due to the interaction and the combined influence of both schools.

We shall see that Nieuwentyt's manner of uniting rationalism and empiricism is to be distinguished from other procedures which are equivalent to a return to the peripatetic conception of science. This latter tendency is particularly strong in German idealism, which sprang from Kant's theory of science; both doctrines represent a return to aristotelian views, superficially adapted to the contemporary stage of scientific development. This will be seen more clearly if we compare Kant's views with the conceptions of an earlier generation.

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[1] A. M. Frenkian, *L'Orient et les origines de l'idéalisme subjectif dans la pensée européenne*, Paris, 1946, I

10 *Nieuwentyt's Theory of Science*

I venture to give, as an illustration of the development of the theory of science about 1700, a summary of the conceptions set forth by Nieuwentyt in his *Fundaments of Certitude, or the Right Method of Mathematicians in the Ideal as well as the Real* (1720). As Nieuwentyt is completely forgotten—I am indebted to Mr. Ph. H. Krijgsman for drawing my attention to his work and for procuring a copy of it—it will be desirable first to give some biographical and bibliographical details.[1]

Bernard Nieuwentyt was born in 1654 in Westgraftdijk, a small village in North Holland between Alkmaar and Purmerend, as the second son of a protestant clergyman. He became a student at the University of Leyden, first in theology but afterwards, owing to a religious crisis brought about by the influence of Cartesian philosophy, he turned to medicine. He took his doctor's degree and settled down in Purmerend, then a town of some importance, as a physician. Related by marriage to the patrician families of the town, he was elected Burgomaster of Purmerend, which he also represented in the States of Holland. He had a controversy with Leibniz about the foundations of the infinitesimal calculus (1695). He died in 1718.

His book on *The Right Use of Contemplating the Works of the Creator* (1716) was a great success. It was translated into English (London 1718; 4th ed., London 1730), French (Amsterdam 1727), and German (Jena 1747), and was greatly admired by Jean-Jacques Rousseau and by Francois-René de Chateaubriand, who even gave, in his *Génie du Christianisme* (1802), a summary of it (tome II, livre V). The *Fundaments of Certitude* which, starting from a methodology of mathematics, attacked Spinoza's metaphysical system, attracted less attention, but nevertheless this book also went through several editions. In this work, Nieuwentyt shows himself to be completely familiar not only with the mathematical and natural sciences of his times, but also with the methodological views of rationalists such as Wolff and empiricists such as Keill.

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[1] For more details, cf. A. J. J. Vandevelde, 'Bijdrage tot de Bio-bibliographie van Bernard Nieuwentyt (1654-1718)', *Verslagen en Mededeelingen der Koninklijke Vlaamsche Academie voor Taal en Letterkunde*, 1926

Nieuwentyt stresses the importance of a clear distinction between the ideal method of pure mathematics and the real method of applied mathematics. Pure mathematics deals with mere ideas. Its criterion of truth is the clear and distinct notion, in full accordance with Descartes' views. However, pure mathematics can teach us no more than the mutual relations of ideas or the mathematical consequences of these ideas, which are chosen at will as a subject of mathematical research. We see that the postulates of deductivity and evidence are satisfied by pure mathematics, at the expense, however, of the postulate of reality. Applied mathematics, on the other hand, has to start from experiences, not from mere ideas or from suppositions springing from bare thought. From the fundamental experiences, applied mathematics takes ideas about real things or about their properties, which are described in definitions, hypotheses, axioms, or however they may be called. If these ideas were perfect and adequate, valid mathematical conclusions from them would be true ; but it is not possible to get perfect and adequate ideas about real things. Applied mathematics therefore has to start from abstract ideas. Mathematical conclusions from abstract ideas are true, but only with respect to the abstract property expressed by the abstract idea, not necessarily if extended to the real thing in its totality. The conclusion may be extended to the real thing in its totality on the basis of experience only. Therefore, in applied mathematics, the clear and distinct idea is no sufficient criterion of truth. So with regard to applied mathematics, the postulate of reality is satisfied, but the postulates of deductivity and evidence are not.

In this connection it is interesting to quote Nieuwentyt's observation, that mathematics and logic differ only as to the exterior form of their argumentation, not as to their conclusive force. Therefore, his analysis holds for any science, not only for mathematical sciences.

Nieuwentyt's methodological views are applied in several discussions concerning speculative metaphysics in general, before he concentrates upon his attack on Spinozism.

It is worth while to mention his criticism of the ontological argument. Here he gives a thorough analysis of sentences such as, ' The idea of a box containing 100,000 guilders implies the idea of 100,000 guilders' ; 'The idea of a triangle implies the idea of three angles' ; 'The idea of a mountain implies the idea of a valley'. It is curious that the very same examples are discussed by Kant in his criticism of the ontological argument, which in all essentials agrees with Nieuwentyt's. It seems possible that Nieuwentyt has been Kant's source, though in the absence of any further information it would be premature to state any definite conclusion. In any case, we are able to conclude that Kant's criticism is built on material which in his times was common property and that it contains no original contribution to the topic. We shall see that the same holds good for Kant's views about the theory of science, apart from certain opinions of his own which lack any solid foundation.

11 *Kant's Theory of Science*

Notwithstanding the customary affirmations to the contrary, we shall see that Kant's obscure and often incoherent—not to say inconsistent—theory of science, as set forth in his *Critique of Pure Reason*, cannot be fully understood without reference to his earlier writings, especially his paper 'On the clearness of the principles of natural theology and ethics' (1764), where he defends a theory of science which in all essentials agrees with Nieuwentyt's views and which may be taken as typical of the period. He distinguishes from the very beginning two fundamentally divergent types of scientific method, which by analogical extension of his own terminology may be called *synthetical* and *analytical* method. Synthetical method corresponds to Nieuwentyt's ideal method, analytical method to Nieuwentyt's real method.

Kant first considers the definitions to which the two methods respectively give rise. A synthetical definition generates a new notion by combining several notions already present ; hence synthetical method deals with ready-made, constructed, notions. In the case of analytical definitions, on the contrary, the notion to be defined is already given beforehand, though confusedly and indistinctly ; an analytical definition cannot create a new notion, it is intended to give an explicit circumscription of a notion already present.

Consequently, synthetical definitions are wholly arbitrary, which is not the case with analytical definitions ; these latter must agree as closely as possible with notions vaguely given beforehand.

A similar distinction holds for methods of proof. We should also distinguish synthetical and analytical reasoning. Synthetically defined notions obviously can be represented by suitable symbols, which by their composition show the manner in which they have been defined. In synthetical reasoning we can safely operate with those symbols instead of handling the notions themselves. In analytical proofs, on the contrary, we must always have in mind the notions themselves, as we can never be quite sure as to the adequacy of the definitions given for them. As Kant puts it : synthetical method considers the general *in concreto*, whereas analytical method is bound to consider it *in abstracto*.

Now of course, the question arises : where did Kant actually find applications of synthetical method ? The answer to this question is at hand : he found synthetical method in mathematics, analytical method in Newtonian physics. His description of synthetical method is strongly influenced by Leibniz' ideas concerning formal logic ; phrases such as : 'substitution of signs according to rules', or : 'instead of the signs themselves, their signs are introduced', could indeed have been written by Leibniz himself.

Kant's problem is whether philosophy should apply synthetical or analytical method. He strongly defends the exclusive application of analytical method in philosophy and rejects the introduction of synthetical method in philosophy by rationalists such as Spinoza. On the other hand, he considers synthetical method as the adequate tool of mathematics and blames Wolff for trying to introduce analytical definitions into this science.

Wolff had, not unjustly, criticised Euclid's manner of defining geometrical similarity, which does not result in a general definition of similarity applicable at once to figures of any sort, but requires us separately to define similarity of triangles, quadrangles, circles, etc. He had attempted to give a general definition of similarity, which he also intended to answer better to the naive acceptation of the notion. Kant rejects these attempts, which he rightly considers as a typical application of analytical method. As a matter of fact, Wolff's attempt turned out to be quite salutary in the further development of mathematics. It gave rise to the introduction of the notion of isomorphy[1] and had another sequel in the group-theoretical foundation of geometry.

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[1] H. Bergmann, *Das Philosophische Werk Bernard Bolzanos*, Halle a. S., 1909 ; 'Zur Geschichte und Kritik der isomorphen Abbildung', *Actes du Congrès international de Philosophie scientifique*, fasc. VII, Paris, 1936

We see that with regard to the methodology of mathematics Kant defends a point of view which essentially coincides with Leibniz's and Nieutwentyt's and which closely resembles the logicist and formalist doctrines in contemporary foundational research.

We now have to deal with the question whether Kant's *Critique of Pure Reason* contains any modification of his original views. A close comparison of relevant passages in his earlier and his later exposition[1] makes clear that Kant's opinions concerning the methodology of mathematics remained untouched but for one single, though essential, point: whereas he originally took synthetical method to start from principles adopted in an entirely arbitrary fashion, in his *Critique* he subjects the application of this method to certain restrictive conditions.

This modification of his earlier views was brought about by Hume's influence. In his precritical period, Kant had adhered to the empiricist thesis, according to which physical science has to restrict itself to analysing experimental results without relying upon the introduction of arbitrary principles such as hypotheses. Now Hume had objected to this doctrine, saying that such a purely analytical procedure could never yield one single causal connection. This formidable objection was fully accepted by Kant. He interpreted it as showing that physical science cannot restrict itself to applying analytical method and that it had to rely on synthetical method as well. This meant, however, that synthetical method could give rise to knowledge about reality, contrary to his original opinion and Nieuwentyt's doctrine.

This insight raises the question as to how synthetical method can deliver knowledge about reality, or, as Kant puts it, 'How are synthetical judgments possible?' His transcendental aesthetic and transcendental logic are intended to constitute an answer to this problem ; this answer consists in subjecting the application of synthetical method to certain restrictive conditions. 'The use of this pure knowledge is based on the condition that we are given objects in intuition to which it can be applied '.[1]

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[1] These relevant passages are: 'Ueber die Deutlichkeit' 1. Betrachtung, § 1: 'Man kann zu einem jeden allgemeinen Begriffe . . . Ich muss ihn zergliedern . . .' *Kritik der reinen Vernunft* (1781) A730: '. . . dass philosophische Definitionen . . . dagegen die ersteren ihn nur *erklären*.'

'Ueber die Deutlichkeit', 1. Betrachtung, § 2: '. . . so berufe ich mich . . . Linien in concreto', 'Vergleicht man hiermit . . . in abstracto erwogen werden'—*Kritik der reinen Vernunft* (1781) A734: 'Selbst das Verfahren . . . vor Augen gestellt wird', 'Da hingegen das philosophische . . . Worte (den Gegenstand in Gedanken) führen lassen.'

'Ueber die Deutlichkeit', 2. Betrachtung: '. . . kann man mit dem Bischof *Warburton* . . . unmöglich kann gebraucht werden'—*Kritik der reinen Vernunft* (1781) A726: '. . . dass die Befolgung . . . Kartenhäuser zu Stande bringe.'

On account of this doctrine, the choice of axioms and fundamental notions in mathematics can no longer be taken to be wholly arbitrary ; the axioms and fundamental notions have to conform to the forms of intuition—space and time ; apart from this condition, however, the development of mathematics can proceed in a purely formal manner, as postulated by Leibniz and Nieuwentyt.

Our interpretation of Kant's methodology of mathematics, though found in a few earlier authors,[2] directly contradicts the one which at present is currently accepted and which especially underlies recent discussions concerning this methodology from the point of view of contemporary foundational research.[3] Applying the terminology accepted so far, we may sum up this current interpretation as follows. By analytical method, Kant means a purely formal or logical procedure, independent of any appeal to extralogical principles ; synthetical method, on the other hand, is characterised by its reliance upon extralogical, especially intuitive and empirical, data.

This interpretation, which I consider as fundamentally incorrect, mainly derives from Kant's observation that the law of contradiction is the source of analytical judgments. I shall have sufficiently dealt with this current interpretation if I show, from my own point of view, what Kant really means by this observation.

Once again I start from Kant's earlier paper. Here he says that analytical judgments are based upon current usage of words and can be detected by means of inner experience. The examples of analytical judgments, given by him in the *Critique of Pure Reason*, confirm this statement. Let us consider the example : ' gold is a yellow metal '.

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[1] *Kritik der reinen Vernunft* (1781), A62

[2] A. Schopenhauer, *Parerga und Paralipomena*, I, *Fragmente zur Geschichte der Philosophie*, § 14 ; F. Ueberweg, *Grundriss der Geschichte der Philosophie*, 6. Aufl., Berlin, 1883, **3**, p. 103, n

[3] G. Frege, *Grundlagen der Arithmetik*, Breslau, 1884, p. 3 ; G. Heymans, *Die Gesetze und Elemente des wissenschaftlichen Denkens*, 4. Aufl., Leipzig, 1923, p. 108 ; M. Schlick, *Allgemeine Erkenntnislehre*, Berlin, 1918, p. 97 ; R. Carnap, 'Der Raum', *Kantstudien*, Erg. h. Nr. 56, 1922 ; E. von Aster, *Geschichte der Philosophie*, Leipzig, 1932, p. 281 ; B. Russell, *A History of Western Philosophy*, London, 1946, p. 706

What Kant means by saying that this assertion is an analytical judgment, can be explained as follows. In order to verify the assertion, I need not consult formal definitions of the terms ' gold ', ' yellow ', ' metal ', or perform logical deductions based upon them, nor am I forced to measure the absorption coefficient of gold for light rays of various wave-length. I can content myself with making sure of the current usages of the words ' gold ', ' yellow ', ' metal ', which can be done by means of inner experiences. We easily see how the law of contradiction comes in : according to current usage of the words involved, it would be a self-contradiction to say, ' gold is not a yellow metal '.

12 *Recent Developments*

Leaving Kant, we may sum up as follows the outcome of the development of the theory of science during the seventeenth century. There are two types of science :

(1) Rational science, which starts from obvious principles and proceeds by rigorous logical deduction and so conforms to Aristotle's postulates of deductivity and evidence, but not to his postulate of reality.

(2) Empirical science, which starts from experimental data and proceeds by analysis.

Consequently, speculative philosophy has to make a choice whether it will be a rational or an empirical science, and it splits up into the currents of rationalism and irrationalism. Kant, by bringing together rational and empirical science, made an attempt to restore, as far as possible, Aristotle's theory of science ; in my opinion, however, he did not succeed. On the contrary, rational science turned farther away from Aristotle's ideal, by dropping his evidence postulate also. The development of non-euclidean geometry constituted the first move in this direction ; the decisive step was taken as a result of contemporary research into the foundations of logic and mathematics. Each of the modern schools in this field—logicism, Cantorism, formalism, and intuitionalism—initially attempted to maintain the postulates of deductiveness and evidence ; they all were forced to drop one of these postulates.[1] An equally significant development can be observed in empirical science. Here, in spite of Mach's phenomenalism, the reality postulate had to be attenuated in order to safeguard the possibility of constructing suitable deductive theories. Modern physical theories do not conform to the evidence postulate, and recently the transition to quantum logic has necessitated a revision of the deductivity postulate.

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[1] This is explained in detail in the author's book on *Les Fondements Logiques des Mathematiques*, Paris, 1950

It is easily understood that these developments have alarmed the representatives of the various schools of speculative philosophy, which, as we have seen, derives its origin, and even its right of existence, from Aristotle's theory of science. This accounts for the violent protestations of speculative philosophers against the developments in modern science, which give rise to theories such as non-euclidean geometry, mathematical logic, the theory of relativity, and quantum mechanics, each of which implies in one respect or another an infringement of the postulates underlying Aristotle's theory of science ; the unanimity of these protestations indeed contrasts strangely with the common discord among speculative philosophers.

To the present writer it seems clear that we are in need of a new and up-to-date theory of science, and that speculative philosophy will have to explore new pathways. Perhaps it may be permissible to consider the Vienna Circle philosophy and existentialism as different attempts to deal with this situation.

Evert W. Beth

DESCARTES AND THE BODY-MIND PROBLEM IN PHYSIOLOGY

1 *Introduction*

THE mode of action of the sense organs, the brain, and the nervous system in general, has always interested philosophers as well as scientists because it is related to the major problem of the relation between the external world and our own knowledge of this world. It is only in comparatively recent times that the aims and methods of science have become quite distinct from those of philosophy. Further, the subject-matter of neuro-physiology, especially with regard to the mode of action of the senses in man, appears at first sight to be something much less objective than the subject-matter of other sciences such as astronomy or chemistry. It is therefore not surprising to find that the separation of scientific from philosophical problems in this field is not always clearly made even today. Studies claiming to be purely scientific are often found, on closer analysis, to involve hidden metaphysical assumptions.

The doctrine of the Cartesian soul residing in the brain, and the mechanistic doctrine, are two incompatible postulates which lie hidden but implicit in the works of many different writers. Sometimes the two different postulates are implied in different parts of the same work. Many physiologists appear in fact to adhere to the Cartesian doctrine, but few state it openly. Others who begin as mechanists seem to feel at some subsequent stage in their reasonings the need to re-introduce something tantamount to the Cartesian soul, although this may be done in a very roundabout manner.

Yet the assumption that the activities of a Cartesian soul in the centre of the nervous system must be taken into consideration, and the assumption that physiology should not deal with such concepts, constitute two completely different starting points. A choice has to be made, which is bound to affect the treatment of neuro-physiological problems. When discussing such problems, it would therefore appear desirable to state explicitly one's method of approach and adhere to it. Even continued failure to solve certain problems may then become a valuable indication, showing that a particular method of approach leads to pseudo-problems and is fruitless.

One object of this paper is to show the hidden influence of Descartes's doctrine in modern physiological reasonings. In the opinion of the present writer and of many others, this doctrine leads to problems intractable by a purely physiological method.[1] Even apart from this, however, the bringing to light of such hidden Cartesianism in physiological works, and elsewhere, may have its usefulness.

2 *The Body-Mind Relationship according to Descartes*

Descartes in 1637 published the *Discours de la Méthode pour bien conduire sa raison, et chercher la vérité dans les sciences, plus la Dioptrique, les Météores et la Géometrie, qui sont des essais de cette Méthode.* The first of the three essays accompanying the *Discourse* is thus *La Dioptrique.* It gives a condensed and lively account of a great number of problems related to the formation of the retinal image, binocular vision, the mode of action and making of spectacles, and other visual problems. In this early work Descartes discusses the relationship between subjective sensations and external stimuli. Thus, the *Dioptrique* gives us, with great freshness and vigour, a first version of Descartes's views on the connection between soul and body.

These views are, essentially, as follows. The soul resides in the brain. The brain receives a pattern of ' movements '—a stimulation pattern in modern terminology—transmitted from the retina, via the optic nerve. This pattern of ' movements ' is then transmitted from the brain to the soul and produces corresponding sensations in the soul. The soul, thus set in motion, acts in turn upon the motor parts of the brain and eventually stimulates the muscles and produces bodily movements. How the soul, or mind, or consciousness, can be affected by the material movements of the brain substance, and *vice versa*, Descartes does not explain.

Up to a point, but up to a point only, Descartes's conception is a mechanistic one. It is concerned with material changes occurring in the retina, the optic nerve, the brain. Here, however, ordinary mechanism stops, and we step from the brain to the mind. Later, we again step from the mind to the motor nervous system.

It may be remarked that the distinction made by Descartes between mind and body is perhaps not always clear cut. For instance, movements of the soul are transmitted to the muscles by ' animal spirits ' flowing inside the nerves, and it is not quite certain whether these spirits are meant to be mind or matter, although the latter is more likely. However, the assumption of intermediate degrees between mind and body would hardly clarify Descartes's views, and it may be doubted whether any man ever understood the workings of such arrangements by which the mind is grafted on to the nervous system.

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[1] Cf. J. H. Woodger, *Biological Principles*, London, 1929 ; M. H. Pirenne, ' On Physiology and Consciousness ', *British Journal of Psychology*, 1947, **37**, 82-86

There is among physiologists no overwhelming majority of opinion in favour of the Cartesian or of the mechanistic doctrine. Thus the pursuit of physiological research does not lead to an obvious choice between the two standpoints. The soul, the mind, consciousness, thought, sensation, being non-material, are not observable in physiological investigation like, say, nerve excitation or muscle contraction. Physiology gives no direct experimental evidence for them. Yet like all men, physiologists no doubt believe that they have minds. Hence a dilemma. On the one hand the physiologist may adhere strictly to his experimental findings, which give to him no direct evidence of mind, and hence jump to the conclusion that there are no minds. This conclusion (unwarranted because there is no proof that physiology gives the whole truth about man) places him who adopts it in the difficult position of denying the existence of his own mind. On the other hand, the physiologist may adhere, on other than physiological grounds, to the widespread view that men have minds. His difficulty then resides in the fact that physiology apparently implies the non-existence of mind or, at least, the non-intervention of mind in physiological processes. As will be explained later, it seems that a dilemma of this kind already faced Descartes. His solution probably appealed to many scientists because it seemed to save the existence of mind, while leaving mechanism to rule over most of the body—which was thus amenable to physiological experimentation. Whatever the intrinsic worth of the line of reasoning sketched in this paragraph, it is a fact that it corresponds to opinions not infrequently expressed by scientists in informal conversation.

3 *The Cartesian versus the Mechanistic Doctrine*

The Cartesian doctrine may be illustrated by the following causal chain :

(A) *receptors—nervous system—*CONSCIOUSNESS*—nervous system—effectors.*

According to this doctrine, the sensory nervous system produces sensations in consciousness and, subsequently, consciousness determines changes in the effector nervous system.

To avoid misunderstanding, it must be pointed out here that certain writers mention consciousness but do not make it an essential part of their theories. They may, for instance, consider it merely as an ' epiphenomenon ' which plays no active role in the sensory process. These writers differ from Descartes in that they consider consciousness as nothing but a kind of onlooker which is entirely by-passed by the nervous processes and which is without influence on them. It is hard to understand what exactly is implied by such a doctrine. So far as physiological investigation is concerned, however, it reduces in effect to the purely mechanistic theory, since consciousness plays no active part whatever in the processes being studied.

In contradistinction to the Cartesian representation, the mechanistic standpoint never deals with consciousness as such, although it will deal, for instance, with the conditions necessary for what we call ' conscious behaviour '. The mechanistic representation may be schematised by the chain :

(B) *receptors—nervous system—effectors.*

4 *The Mechanistic Standpoint as a Methodological Postulate*

A completely mechanistic representation of the workings of the nervous system avoids, of course, the difficulty of the body-mind relationship, since it disregards the mind altogether.

The chain :

(B) *receptors—nervous system—effectors*

does not contain any link between nervous activity, which is directly observable, and consciousness, which is not. Physiologists who declare themselves materialists because they have never discovered the soul under their scalpel are, in their capacity as physiologists, undoubtedly right. Without attempting to give a definition of the soul, mind, or consciousness, it may be argued that one of its main characteristics is precisely that it is incapable of being demonstrated in this way.

Even those who would not accept the mechanistic standpoint as the only rational one in physiology will probably agree that such a standpoint can be adopted as a working hypothesis. It is then worth examining what exactly it implies, for the Cartesian doctrine has a way of creeping into the reasonings even of professed mechanicists and materialists.

The mechanistic conception of the working of the nervous system might be briefly described as follows : physico-chemical stimuli acting upon receptors determine excitations in these receptors. These excitations spread into the nervous system, including the brain. They interact in the nervous system in ways which must be very complicated and are as yet unknown, with static or dynamic changes having other origins. These changes include those due to stimuli which have ceased to act, as well as those due to events which affected only the remotest ancestors of the organism considered. (Thus the facts related to ' memory ', ' habit ', ' heredity ', etc., are, theoretically at least, taken into account.) Eventually, as a result of these interacting nervous activities, a certain spatial and temporal pattern of excitation is transmitted from the nervous system to certain muscles and determines a definite behaviour of the organism.

As an example, consider a look-out placed by housebreakers at the corner of a street. Seeing the outline of a policeman in the distance, the look-out runs silently towards his confederates to warn them. The mechanistic interpretation here should be given purely in terms of neuro-physiology, neuro-anatomy and physics, and it should account for the complete behaviour of this man. In a mechanistic explanation, it would be no use saying that the look-out *knows* that shouting would attract the policeman's attention, for ' knowing ' is a property of the mind, not of the nervous system. The same applies to all psychological concepts such as ' seeing ', ' remembering ', ' warning '.

The mechanistic standpoint considers the human organism as behaving in a way similar to that of machines such as the automatic pilot of an aeroplane, or the automatic fire and burglar alarm system of a bank. The behaviour of such machines is determined by the way they are built, by the physical stimuli which they receive at the time under consideration, and by their past history. While the tasks accomplished by machines are much simpler than those of our look-out, the construction of an automaton which would *behave* like the look-out is not inconceivable. But of course the construction of an automaton which could behave exactly like the look-out in any circumstances including, for instance, a fight with the police, would *in practice* meet insuperable difficulties.

However, we are concerned here only with the theoretical possibility of such an automaton. It is no argument against the mechanistic standpoint to say that any existing robots are extremely clumsy and imperfect compared even to an animal such as a dog. Some physicists have discussed the evolution of the whole universe considered as a material system which changes according to the laws of physics. Such a theoretical conception obviously includes in its scope all the living organisms, which are thus *ipso facto* considered from the mechanistic standpoint discussed in this paragraph.

In all this, it must, of course, be borne in mind that the subject-matter of physiology is how the organism *actually* works. Now if it is possible to build an automaton behaving in a certain manner, it is in principle possible to make any number of automata, all built in a different way, performing the same task. The existence of a machine behaving like a certain organism therefore does not in any way prove that the organism actually works like this machine. It is universally recognised that the incautious use of physico-chemical models for explaining the workings of biological systems is beset with pitfalls.

5 *The Soul–Body Relationship of Descartes in Modern Statements*

Descartes expressed himself in a concrete and forcible manner. As a result, some at least of the defects and difficulties of his doctrine were easy to detect in his own time. Such a critic as Gassendi[1] was quick in pointing out to Descartes that it was not possible to understand how his body-mind could work.

Three centuries after the publication of the *Discourse*, however, what first strikes the modern reader is the quaintness of the details of Descartes's physiological imaginings. The soul resides in the pineal gland at the centre of the brain. There it receives sensory messages through a system of very fine wires running inside the thin tubes composing the optic nerve. These wires are pulled to and fro by the impact of light on the end-organs in the retina. The muscles are brought into action by animal spirits reaching them from the pineal gland through the fine tubes constituting the nerves. (Descartes erroneously believed that all nerves are both sensory and motor.)[2]

The modern reader who thinks he can dismiss the Cartesian doctrine as fantastic may, however, be dismissing only the obsolete scientific details while retaining the substance of the theory. Today statements such as the following are commonplace: 'The stimulation of the retina by light causes the stimulation of the optic nerve, and when the latter stimulation is relayed to the visual cortex, or to a certain layer of this part of the cortex, a visual sensation arises in consciousness'. Such statements do not as a rule cause surprise or meet with contradiction. Yet we are dealing with views which are essentially similar to the Cartesian doctrine or at least to the main part of it. The fact that such statements are put in modern language, with anatomical and physiological details up to date, does not hide this similarity once attention has been drawn to it.

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[1] 'Cinquièmes Objections, faites par Gassendi contre les six Meditations [de Descartes].' See especially 'Contre la Sixième Meditation [entitled] De l'existence des choses matérielles et de la distinction réelle entre l'âme et le corps de l'homme'.

[2] See *Dioptrique.*

Such statements do not always go on to say what events take place after the sensation has arisen in consciousness. The end constituents of the complete Cartesian chain are often missing. Instead of the complete Cartesian chain:

(A) *receptors* — *nervous system* — CONSCIOUSNESS — *nervous system* — *effectors*

we have:

(*a*) *receptors*—*nervous system*—CONSCIOUSNESS.

Chain (*a*) is incomplete yet it implies the complete chain (A) above, for consciousness, sensations, ideas cannot be directly observed and experimented with like nerve or muscular activity. Behaviour, not sensation, is the physiological response to stimulation of the sense organs. This point will be discussed in the next paragraph.

6 *Behaviour and Sensation*

Consider, for example, a simple experiment on the threshold of vision, in which the minimum amount of light which can be seen by a man under given conditions is to be determined. The human subject of the experiment must indicate at each trial whether he has seen the test-light or not. He may speak and say 'Seen' or 'Not seen', or he may merely press a key according to a prearranged convention. He may conceivably memorise the results of the experiments and write them down later. But the essential point is that the subject of the experiment must *do* something which indicates whether or not he has had a sensation of light. Otherwise no analysis of the experiment is possible. There would probably be no argument on that count if the subject of the experiment were an animal, but it is no less true in the case of a human subject.

A critic might object: 'But *I* observe *my own* sensations directly'.

This is no way out of the difficulty. For if any scientific study is to be made of the critic's introspective observations, these observations must be expressed in some way, if only by the merest gesture. There must be some objective behaviour of one kind or another. Literary history cannot take into account the 'mute inglorious Miltons' so long as they remain genuinely and completely 'mute'. Neither can sensory physiology take into account the sensations of subjects who fail to communicate in some way, if only unwittingly, what their sensory experience was during the experiments. Thus what physiology must first attempt to explain is the directly observable objective response by muscles or glands to stimuli acting on sensory receptors of an organism. If a theory dealing with both subjective sensations and objective behaviour leads to wrong deductions with regard to behaviour, it will be trusted in neither respect.

Physiologically speaking, therefore, it appears unsound to say that the response of the sensory nervous system to stimulation of some of its receptors is a sensation, in contradistinction to the response of the motor system which is muscular activity. In an intact organism the response to stimulation of the sensory receptors is a certain behaviour bringing into action or putting out of action ordinary effectors such, for instance, as the muscles involved in speech.

7 *Some Physiologists who followed Descartes's Doctrine*

The Cartesian interpretation of the working of our mind and senses is at the present time quite commonplace. Many, whether scientists or laymen, take it simply as a scientific fact. It has entered ordinary conversation. While some will laugh at the notion of a little soul confined to the pineal organ of the brain, they will speak without hesitation of 'getting an idea into someone's head'. This is often much more than a vague metaphor. They will be surprised to hear serious doubts expressed as to whether the idea is really located in the person's brain.

Among scientists Johannes Müller and Helmholtz may be counted as followers of Descartes. Johannes Müller based his famous principle of the 'specific energy of nerves' on the implicit assumption of the chain (*a*) above:

(*a*) *receptors—sensory nerves—brain*—CONSCIOUSNESS

which corresponds to the first part of the Cartesian system. Helmholtz concluded from J. Müller's principle that the nature of our representation of the outside world can bear no relation to the nature of the outside world.[1]

The essentials of both Müller's principle and Helmholtz's epistemological views had been stated concisely but forcefully by Descartes himself two centuries earlier.[2] Curiously enough Descartes does not

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[1] A fuller discussion of the views of J. Müller and Helmholtz will be found in M. H. Pirenne, *British Journal of Psychology*, 1947, **37**, 82-86. The main references are : Johannes Müller, *Handbuch der Physiologie des Menchen für Vorlesungen*, 2 vols., Coblenz, 1833-1840 ; H. von Helmholtz, *Handbuch der Physiologischen Optik*, 1st ed., Leipsic, 1867 ; H. von Helmholtz, 'Die neueren Fortschritte in der Theorie des Sehens' (reprinted in *Vorträge und Reden*, 4th ed., vol. i, pp. 265-365, Braunschweig, 1896 ; translated as 'The recent progress of the theory of vision' in *Popular Lectures on Scientific Subjects*, 1st ser., pp. 175-276, London, 1904) ; H. von Helmholtz, *Die Tatsachen in der Wahrnehmung*, Berlin, 1879 (reprinted in *Vorträge und Reden*, vol. ii, pp. 213-47) ; H. von Helmholtz, *Schriften zur Erkenntnistheorie*, edited with commentary by P. Hertz and Moritz Schlick, Berlin, 1921.

[2] 'Au reste, les images des objets ne se forment pas seulement ainsi au fond de l'oeil, mais elles passent encore au-delà jusques au cerveau . . . ; d'où il est manifeste qu'il se forme derechef une peinture 789, assez semblable aux objets V X Y, en la superficie intérieure du cerveau qui regarde ses concavités ; et de là je pourrais encore la transporter jusques à une certaine petite glande qui se trouve environ le milieu de ses concavités, et est proprement le siège du sens commun. . . .' *Dioptrique*, end of Discours Cinquième.

'Or, encore que cette peinture, en passant ainsi jusques au dedans de notre tête, retienne toujours quelque chose de la ressemblance des objets dont elle procède, il ne se faut point toutefois persuader, ainsi que je vous ai déjà tantôt assez fait entendre [*Dioptrique*, Discours Quatrième], que ce soit par le moyen de cette ressemblance qu'elle fasse que nous les sentons, comme s'il y avait derechef d'autres yeux en notre cerveau avec lesquels nous la puissions apercevoir ; mais plutôt que ce sont les mouvements par lesquels elle est composée, qui agissant immédiatement contre notre âme tant qu'elle est unie à notre corps, sont institués de la nature pour lui faire avoir de tels sentiments, ce que je vous veux ici expliquer plus en détail. Toutes les qualités que nous apercevons dans les objets de la vue peuvent être réduites à six principales, qui sont la lumière, la couleur, la situation, la distance, la grandeur et la figure. Et premièrement touchant la lumière et la couleur qui seules appartiennent proprement au sens de la vue, il faut penser que notre âme est de telle nature, que la force des mouvements qui se trouvent dans les endroits du cerveau d'où viennent les petits filets des nerfs optiques, lui fait avoir le sentiment de la lumière, et la façon de ces mouvements celui de la couleur, ainsi que les mouvements des nerfs qui répondent aux oreilles lui font ouïr les sons, et ceux des nerfs de la langue lui font goûter les saveurs, et généralement ceux des nerfs de tout le corps lui font sentir quelque chatouillement quand ils sont modérés, et quand ils sont trop violents, quelque douleur, sans qu'il doive en tout cela y avoir aucune ressemblance entre les idées qu'elle conçoit et les mouvements qui causent ces idées : ce que vous croirez facilement, si vous remarquez qu'il semble à ceux qui reçoivent quelque blessure dans l'oeil qu'ils voient une infinité de feux et d'éclairs devant eux, nonobstant qu'ils ferment les yeux ou bien

seem to have been acknowledged by J. Müller and Helmholtz as a precursor, but no doubt it is only indirectly that his views influenced theirs.

Hermann Aubert, author of the *Physiologie der Netzhaut*, published in 1865, was a straightforward follower of Descartes and must be mentioned here. His *Physiology of the Retina* is based on what he calls the 'Kant-Müllerschen Standpunkt', but this standpoint is essentially the Cartesian doctrine. Aubert defines his philosophical starting point in the introduction and adheres to it consistently through the book, saying that it will have to be judged by its fruits. While there is much in the field he covers which is relatively independent of the standpoint chosen, this is not the case for theories of binocular vision and of colour vision. Aubert's critical discussion of these topics on strictly 'Kant-Müller' lines is illuminating and shows the difficulties into which he is led by this method of approach. At one point he utters what seems to be a cry of despair which may find an echo today in the breast of those who begin the study of colour vision theories: 'What does this really mean: mixture of sensations?' 'Was heisst überhaupt: Mischung von Empfindungen?'[1]

8 *Physiological versus Psychological 'Integration'*

Words such as integration, synthesis, fusion, may be used in physical as well as in mental contexts. Such double usage has sometimes led to serious confusion which, in certain aspects, is related to the present discussion of the Cartesian doctrine. The word 'integration' in particular has come into much use lately. Its meanings and their implications may be briefly examined in connection with our present problem.

The phrase 'integrative action of the nervous system', as used by Sherrington, describes among other things the co-ordinated functioning of the various muscles involved in a complex motor action, such as the flexing of a limb. Integration here may be taken to refer to physical

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qu'ils soient en lieu fort obscur ; en sorte que ce sentiment ne peut être attribué qu'à la seule force du coup, laquelle meut les petits filets du nerf optique, ainsi que le ferait une violente lumière ; et que cette même force, touchant les oreilles, pourrait faire ouïr quelque son ; et touchant le corps en d'autres parties, y faire sentir de la douleur.' There follows along similar lines a discussion of the after-images which we see after looking at the sun or at another bright source of light. *Dioptrique*, beginning of Discours Sixième. (Original spelling modernised)

[1] *Physiologie der Netzhaut*, Breslau, 1865, p. 181

events only. A smoothly-working automaton may without ambiguity be described as a well-integrated piece of mechanism, its 'integration' being due to suitable regulating devices which may, or may not, have characteristics resembling those of the human nervous system.

Another—perhaps the first and the simplest—meaning of the word 'integration' occurs, of course, in mathematics. A mathematical integral is a sum of infinitesimal parts defined according to a certain law. In sensory physiology, stimuli acting at the same time on a number of receptors cause individual stimulations in these receptors. These individual stimulations may, so to speak, help each other so that their combined action leads to a response which any one of the stimulations acting alone would have been incapable of determining. This as a rule will be due to the convergence of individual nervous excitations on to a 'final common path'. A simple analogy comes to mind, namely that of the funnel used in meteorological studies in order to collect rain water in easily measurable quantities. Such processes of summation—although their mechanism may be very complex [1]—have a definite resemblance to mathematical integration and may thus also be described as cases of physiological integration.

The word integration, however, is also used in such phrases as a 'well-integrated mind' when referring to a man in whom, for instance, emotion and reason are well balanced. Here 'integration' refers to psychological qualities, not to physiological mechanisms. Other types of mental integration may be considered, for instance the integration of various ideas into a coherent theory.

Thus we have on the one hand various kinds of mental or psychological integration, and on the other hand we have two types of physiological or physical integration. An example will show how confusion can arise when conclusions about the mental integration are drawn from consideration of physiological integration.

9 *Integration and Discrimination in the Visual Process*

Let us consider one detail of Johannes Müller's theory of the vision of insects and arthropods.[2] Müller was the first to explain the mode of action of the compound, facetted, eyes of such animals. To each facet corresponds what is now called one *ommatidium*. This, according to Müller, consists essentially of an elongated cone whose walls contain black pigment and whose base, the facet, is transparent and turned towards the outside world. The tip of the ommatidium therefore can receive light from one restricted region of space only.[1] For each facet, for each ommatidium, there is one single nerve fibre. The light reaching the tip of one ommatidium acts upon the photo-sensitive end-organ connected to the nerve fibre and stimulates it. Each nerve fibre is thus stimulated to a greater or lesser extent according to the amount of light received by the tip of its corresponding ommatidium. A spatial pattern of stimulation related in a definite manner to the distribution of light in the outside world is formed in the mosaic of ommatidia and nerve fibres. Such an arrangement is incapable of recording details smaller than the region of space corresponding to one ommatidium, just as a mosaic is incapable of representing details smaller than one of its constituent stones.

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[1] Cf., for example, M. H. Pirenne, *Research*, 1949, 2, 259–266.

[2] Johannes Müller, *Zur vergleichenden Physiologie des Gesichtssinnes des Menchen und der Thiere, nebst einem Versuch über die Bewegungen der Augen und über den menschlichen Blick*, Leipsic, 1826

Now J. Müller wished to explain how, although the periphery gives such a mosaic of discrete stimulations, the animal could see a smooth continuous picture of the outside world or, in other words, how the individual stimulations could be *integrated* into a unified perception. He wrote : ‘ Each optic nerve fibre with its particular visual impression communicates this impression to the ganglion of the optic nerve. The optic fibres, previously separated by the pigment [of the walls of the ommatidia], become united into the ganglion of the optic nerve, leading to a continuous broadening of the nervous mass as they come together. In the same way as this occurs, the single impressions of the single optic fibres also become united in the ganglion to form a continuous image in consciousness ’.[2]

J. Müller here uses a causal chain starting with an optical apparatus and leading to impressions (‘ Eindrücke ’) in consciousness (‘ Affection ’). The demarcation between what is nervous and what is mental remains somewhat vague, but Müller's standpoint nevertheless is very reminiscent of Descartes.

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[1] J. Müller's original diagram is reproduced in M. H. Pirenne, *Vision and the Eye*, London, 1948, p. 102

[2] ‘ Jede Sehfaser mit ihrem besondern Gesichtseindrucke theilt diesen dem Bulbus des Sehnerven mit ; und indem sich die früher durch Pigment getheilten Sehfasern zum Bulbus des Sehnerven vereinigen, zu einer continuirlichen Ausbreitung der Nervenmasse zusammentreten, so verbinden sich auch die einzelnen Eindrücke der einzelnen Sehfasern in dem Bulbus des Sehnerven zu einem continuirlichen Bilde in der Affection.’ J. Müller, *Zur vergleichenden Physiologie des Gesichtssinnes* . . . , p. 365

The modern objection to Müller's theory is an obvious one, though we must remember that we come a century after his great pioneer work and we have been helped by his very simplicity and force of expression. The objection is that this fusion of the nerve fibres which Müller assumed is very similar to convergence on to a final common path and would make discrimination between different retinal patterns of stimulation impossible. If the nerve fibres really became united into a homogeneous mass, the nervous stimulation at any subsequent stage would have the same characteristics whether the animal was looking at a bright circle or at a bright square of the same area. Similarly, the amount of rain water entering the flask of the meteorologist would tell him nothing about the shape of the funnel but only about the area.

We meet here in an acute form the dilemma of physiological and mental integration. Physiological integration in the sense of summation works against discrimination. The mechanism of *physiological* integration invoked by Müller to give an integrated *mental* image to his insects would lead to the complete loss of the ability to respond differently to different patterns.

While from a purely mechanistic standpoint Müller's explanation is certainly absurd, it may not be so from the Cartesian standpoint, disregarding the fact, irrelevant here, that Descartes himself considered animals as mere automata and applied his theory of the soul to man only. A Cartesian might argue that the consciousness of the insect receives an image of the outside world, that it is affected in a certain way by this image, and that as a result the consciousness sends a certain kind of stimulation to the effector nervous system. Then the above mentioned objection, based on the idea of the final common path, does not strictly apply. It is not clear, however, why the image seen should be smooth in this case, and it is uncertain whether Müller himself would have accepted such a doctrine in full. What is certain is that Müller's crypto-Cartesianism has led him to formulate an obscure theory disproved by anatomical facts, for the optic ganglion is not a homogeneous 'nervous mass'.

Similar perplexities may occur when reading modern writers. For instance, 'Gestalt' psychology is interested in the perception of continuous wholes as a result of a 'mosaic' of peripheral stimulations. It is hard to dispel the lingering suspicion that some of the theories advanced to explain such perception are somewhat similar to Müller's theory. More generally, statements of the following kind will be met: 'The pattern of the external environment requires first to be analysed by peripheral receptors and then to be resynthesised by the activity of the cerebral cortex'. What such a resynthesis consists of is hard to understand unless it is a mental synthesis, in which case we are faced with the Cartesian doctrine once again.

10 *The Mechanistic Approach in Sensory Physiology*

Consideration of the mechanistic method of approach in physiology shows it to be logical and consistent. We start, for instance, by studying the working of the optical system of an eye, be it the compound eye of an insect or a vertebrate eye like that of man. We then consider the excitation produced by light in the individual visual cells. We follow this excitation as it is propagated in the nervous system from neurone to neurone. At this stage interaction probably occurs, in an extremely complex manner, with excitations of different origins. Eventually nervous excitation reaches, as a rule, some effector mechanism and brings it into action. Theoretically, all this could be directly observed, complicated though it is, if our methods of investigation were sufficiently refined and powerful.

Sensations, consciousness, the mind, the soul, are not met with in the course of such investigations. We may therefore say that physiology, *qua* physiology, does not deal with such concepts. They belong to psychology or philosophy. Physiologists who introduce such concepts in their reasonings become *ipso facto* psychologists or philosophers.

It may then be of some interest to examine the purely physiological, mechanistic, aspect of certain problems which have occupied psychologists. We may do this, for example, with the problem of 'stimulus equivalence'. We are generally able to identify a square as a square, whatever the size, colour, position, etc., of the particular square object which is presented to us. From the mechanistic point of view, this means that our organism can give a constant response to many different stimuli which have in common only the characteristic of being square. On the other hand, to any of the members of another series of stimuli whose only common characteristic is that, say, of being round, our organism will give another, constant response.

Lashley[1] has considered this problem in connection with the theory of conditioned reflexes, showing incidentally the high complexity of the physiological situation even in apparently simple cases of this type.

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[1] K. S. Lashley, 'The Problem of Cerebral Organisation in Vision,' *Biological Symposia*, 1942, 7, 301

As is well known, a square image formed on the retina and stimulating the photo-receptors determines a pattern of excitation which is eventually relayed, via the optic nerve, to the visual area of the cerebral cortex. The pattern of excitation reaching the surface of the cortex retains roughly the shape of the original square stimulus. It is very likely that a spread of excitation from each point of this primary cortical pattern to neighbouring points of the cortex occurs, but at this stage definite physiological information is lacking. Now Lashley suggests as an hypothesis that the interplay of this spreading excitation causes a new excitation pattern, by a mechanism resembling the formation of interference patterns in wave-systems. This secondary pattern spreading into the cortex is as a rule entirely different in shape from the primary cortical pattern, but it will always be the same whenever the primary cortical pattern is a square. Similarly, when any circular primary pattern reaches the cortex, a characteristically different secondary pattern will be formed. Thus the same characteristic kind of excitation will be produced, at a certain level of the nervous system, when the retina is stimulated by any square image. Then, of course, it follows that this secondary excitation will be capable of determining a response which will also be the same for each and every square stimulus. This suggestion of Lashley is only a working hypothesis. It is nevertheless sufficient to show that a purely mechanistic interpretation is, in principle, capable of explaining the ability of the organism to give a constant, appropriate, response to different stimuli which have only one single characteristic in common.

Such problems have, perhaps, not yet received all the critical attention which they deserve. Many authors have lately made comparisons between the brain and certain calculating machines. It seems, however, that some of them have been keener to demonstrate that certain brain mechanisms may be similar to those of the calculating machines than to find out what actually occurs in the brain and nervous system. Discussions in general terms of very complex problems have taken place. Their exact bearing on physiology is not always easy to assess. Curiously enough, the simplest problems seem to have been studied least of all.

For example: What is the nervous mechanism which causes the organism to give consistently a different response when, say, a square or a round patch of light is presented to the eye? The simplest experimental conditions may be assumed in this case in order to reduce to a minimum the problem of ' stimulus equivalence '. The habit of thinking along more or less Cartesian lines, as in the case of Johannes Müller, may have obscured the existence of such simple problems. Yet they are well worth considering at an early stage, in order to go from the simple to the complex. For it would appear to be *practically* impossible to deal in a detailed mechanistic way with certain problems of behaviour, on account of the immense complexity of the processes involved.

II *Descartes's Dilemma*

Descartes regarded animals as mere machines, as automata of superlative efficiency.[1] It may be surmised that he who had built his philosophy on the axiom ' *I think*, therefore I am ', felt it impossible to regard his own person as nothing but an automaton. For a pure automaton, it can legitimately be argued, cannot think. Thus if Descartes had adopted the mechanistic view as the whole truth about himself, he would have denied his own thinking. According to his axiom, he would therefore have denied his very existence.

Thus, although Descartes's doctrine of the soul residing in the human brain and interacting with the nervous system may strike us as strange and incomprehensible, he may have been driven to it on purely logical grounds, as being apparently the only way out of his dilemma. He regarded it as an improvement on the views of other philosophers who assumed that the soul was able to contemplate images formed in the brain.[2] Having accepted the above doctrine as far as his own person was concerned, it was then a natural step for Descartes to extend it to all men.

We are still faced today with a similar difficulty. As philosophers or, simply, as men, we can hardly deny the existence of our mind. For if our mind does not exist, then our ideas and theories have no existence either. Now these theories include the very theory that the mind does not exist. If we accept the mechanistic standpoint as giving the whole truth, we are caught in a circle of contradictions.

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[1] Cf. letter to the Marquis of Newcastle, 23rd November, 1646

[2] ' Il faut, outre cela, prendre garde à ne pas supposer que, pour sentir, l'âme ait besoin de contempler quelques images qui soient envoyées par les objets jusques au cerveau, ainsi que font communément nos philosophes. . . .' *Dioptrique*, Discours quatrième.

This refusal to deny the existence of one's mind seems to be the main reason why so many physiologists are reluctant to adopt a purely mechanistic standpoint and why they adhere, sometimes almost surreptitiously, to some form of the Cartesian doctrine.

However, we are under no compulsion to believe that physiology gives us the whole truth about life and about ourselves. Physiology can be taken as referring only to certain aspects of reality. If we reserve our judgment as to the ultimate value of physiological findings, there is no difficulty in adopting the mechanistic standpoint as a methodological postulate, even in the field of human sensory physiology. The Cartesian links between mind and body, which are not amenable to physiological investigation and whose introduction into physiological reasonings causes such confusions and obscurities, are thus avoided altogether.

Such a method of approach does not deny the existence of the mind. It simply decides from the start not to deal with the mind, but only with the body. The fundamental problem of the body–mind relationship then remains, but as a metaphysical problem, not as a scientific one. With regard to this metaphysical problem, doubts about the adequacy of the Cartesian doctrine as a solution may again be entertained, but such enquiries are beyond the scope of the present paper.[1]

M. H. Pirenne

[1] I am greatly indebted to many friends for discussions on the above problems and I should like here to thank particularly Professor E. F. O'Doherty, and Dr A. C. Crombie

METAPHYSICAL INTERPRETATIONS OF SCIENCE

PART I

1 *Introduction*

If we apply to metaphysics the criteria of truth which have been accepted generally in modern science, we can conclude, on good grounds, that tenets of metaphysics (e.g. reality of the external world, mental character of the universe, etc.) are neither true nor false, but meaningless. Although this argument can hardly be refuted, the interest in metaphysics has abated very little. It is claimed that the concept of truth which has been accepted by science does not provide the only valuable kind of truth. Metaphysics may be meaningless for the scientist ' as a scientist ', but may be of the highest value for human life.

In regarding metaphysics as a trait of human behaviour, there is no doubt that people who advocate metaphysical tenets do it with great fervour. Moreover, there is no doubt that often it has been very practical to stand for specific metaphysical principles ; they have exerted strong influence on the individual and social behaviour of men. To agree on this point, we need only to consider the ' cold war ' which is now going on and is threatening our civilisation with destruction. Both parties in this ' war ' have stressed that an agreement is impossible because the conflict has its roots in opposing ' philosophies '. In the political debate we hear often that all issues might be resolved by peaceful negotiation if the ' materialistic creed ' of the Soviet government did not frustrate every attempt towards understanding. This creed is often called a ' materialistic metaphysics ', and occasionally a ' lack of metaphysics '. The creed of the Western democracies is frequently referred to as ' idealistic '. This word has, on one hand, a laudatory connotation, while in the language of Soviet philosophy the word ' idealistic ' means a method of deceiving people by empty talk about a spiritual world.

From this ' practical ' use of metaphysics, quite a few authors have inferred that the statements of metaphysics do not tell us anything about the objective world and that metaphysics has no ' cognitive ' value whatsoever. It is rather a method of education intended to influence the behaviour of people, a kind of high-level propaganda talk. To a certain degree, one could invoke for this conception the authority of a philosophical classic like Kant, who did not regard metaphysical statements as a product of ' pure reason ' but found a niche for them in his critique of ' practical reason '. This role of metaphysics in ' practical ' life has certainly contributed much to the high esteem in which it has been held. In this paper we shall deal only occasionally with this side of the problem. We shall try instead to find out what statements of metaphysics mean if we regard them as statements about objective facts, and we shall investigate typical metaphysical statements in order to find out what testable facts they are asserting.

The case for the ' meaninglessness ' of metaphysical statements, according to accepted scientific criteria of truth, can be made easily. We mean by a ' science ' (e.g. physics) a system of general principles from which, by chains of logical conclusions, statements about observable facts can be derived. These facts are then checked by observation and experiment.

The derived facts in a scientific system are formulated in the language of sense observation (e.g. on that place at that time is a brown spot). This is the language which we use in our every-day life to describe, e.g. our breakfast table. Every experiment in nuclear physics or any observation in astrophysics can be described in this language which we may also call the language of ' common sense ' ; however, the *principles* of science contain terms of a very different type. They contain words and terms like ' electromagnetic field strength ', ' gravitational tensor potential ', etc. They are expressed frequently by mathematical symbols and are generally very remote from the terms of commonsense language. These symbols do not designate any observable properties of bodies, but by logical deductions from these principles and the relations between these symbols and sense experiences, statements about observable facts can be derived. If this is so, the principles can be checked by actual experiment or observation and in this case we call the principles ' scientific principles '.

If no statement about observable facts can be derived we say that these principles belong to metaphysics, as, e.g. the statement that ' the external world is really existent and essentially mental ' or, for that matter, ' essentially material '. According to the generally accepted standards of truth in science, such metaphysical statements are neither true nor false. They are called ' meaningless according to scientific standards '. It seems obvious that we can establish in this way a clear-cut distinction between scientific and metaphysical principles. If a statement which claims to give a ' metaphysical implication of science ' can be checked by the agreement of its consequences with experience we have to regard these would-be metaphysical implications as part of science. The same distinction can also be made among the statements which are commonly called ' ethical ' or ' aesthetical ', briefly speaking among value judgments. If they can be checked by observation, they are to be classified as scientific statements. But if this is not the case, they belong to metaphysics. Occasionally the term ' metaphysics ' has been used in such a broad sense that every statement about quantities which are not directly observable has been called ' metaphysical ', even if it can be checked by experiment or observation. However, we shall include such principles in science and reserve the name ' metaphysics ' for statements and interpretations which do not add to a scientific theory anything which would improve the agreement with observational facts.

Perhaps we can find out the standard conception of what the purpose of metaphysical interpretations is by consulting the article ' Science ' in the *Encyclopedia Britannica.* The author points out that when we have pursued science as far as we can go, we must face an ' inquiry into the ultimate nature of reality . . . but this inquiry lies in the province of metaphysics and is not necessarily involved in the pursuit of natural science. Metaphysics uses the results of natural science . . . as evidence bearing on her own deeper and more difficult questions.'

Following the above line, we shall understand—in this paper—by a ' metaphysical interpretation of science ' a search for the reality behind the physical phenomena. We can describe this task in a more specific way if we use the formulation given by the German physicist Max Planck [1] who believed strongly in a metaphysical interpretation of physics. Planck started from the obvious fact that every physical theory contains two types of terms : the terms denoting sense observations (red, warm . . .) and the symbols forming the general principles (potential, force . . .). The disbeliever in metaphysics, the ' positivist ', as Planck calls him, claims that beyond these two realms of concepts nothing can be said about physical science ; but the adherent of a metaphysical interpretation assumes, as does Planck, a

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[1] I discussed Planck's formulations in a paper in the Italian journal *Scientia.* It is reprinted in my collection of essays *Between Physics and Philosophy*, Cambridge, Mass., 1941.

'third realm' which tells us about the 'physical reality' behind the symbols and behind the sense observations.

One could define the 'metaphysical interpretation of science' as a system of statements about this 'third realm'. It has always been a cumbersome question regarding what language this third realm, this 'realm of reality', could be described in, since one can use neither the language of sense impressions nor the language of physical symbols. We see clearly in Planck's latest formulation of his views that nothing actually can be said about this reality except that it 'exists'. It has become very clear to Planck that there is no language in which this reality can be described.[1] He writes, 'There is a metaphysical reality behind everything that human experience shows to be real . . . Metaphysical reality does not stand spatially *behind* what is given in experience, but lies fully *within* it. . . . There always remains a gaping chasm, unbridgable from the point of view of exact science, between the real world of phenomenology and the real world of metaphysics.' Planck thinks that we approach this metaphysical reality by 'drifting farther and farther away from the immediate facts . . . to less intuitive trails.' The concepts which science uses in its general principles are the nearest to reality that man can reach. Of this reality we know only that it exists, but we cannot describe it.

We can interpret this view, which is quite common among scientists, in two ways. If a scientist is 'tough-minded' (to use an expression of William James), he will say that by the 'existence of a physical reality behind the phenomena' he means exactly that there are 'uniformities among these phenomena' which can be treated by experience. The restrictions imposed by these experiences upon any hypothesis can be called the 'reality behind the phenomena'. If the scientist is rather 'tender-minded' in William James's sense, he will say that a 'reality exists behind the phenomena' but cannot be described by words. This is tantamount to Herbert Spencer's assertion about the 'Unknowable', a kind of minimum metaphysics within his empirical philosophy. But even such a minimum seemed to Herbert Spencer and, for that matter, to Max Planck, to be sufficient for use as a bridgehead to pass into the field of traditional religion and to achieve the practical purpose of metaphysics, the influence on human behaviour.

However, the full-blooded adherent of metaphysics would not be satisfied by this narrow stretch of ‘ Unknowable.’ He would like to give an elaborated description of the reality behind the phenomena, not only an assertion of its bare existence. We have to investigate how the searchers for ‘ ultimate reality ’ have attempted—in the history of human thought—to overcome the basic difficulty : to find a language in which this reality can be described precisely or, at least, vaguely. A prominent advocate of Thomistic metaphysics, G. B. Phelan,[1] writes about the difficulty in describing adequately ‘ reality itself ’ : ‘ When the intellect of man is confronted with *being as being* it is dazzled and can only hope to see it by gazing upon its analogical reflections or participations in the things that are.’ This means, approximately, that the ‘ ultimate reality ’ can be described only by an analogy which is formulated in the language of our every day experience. Phelan quotes the scholastic philosopher Cajetan who said ‘ without an understanding of the doctrine of analogy it is impossible to acquire a knowledge of metaphysics.’ We shall discuss this doctrine later in more detail ; but we may note for our preview that the most elaborated system of metaphysics, the Thomistic system, has understood for ages that the ultimate reality cannot be described in specific technical terms, but only by using analogies in the language of our everyday life. In other words, a ‘ metaphysical interpretation of science ’ makes use of analogies taken from commonsense knowledge.

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[1] M. Planck, 'The Meaning and Limits of Exact Science', in *Scientific Autobiography and Other Papers*, Philosophical Library, New York, 1949, pp. 80 ff.

We can understand this point very easily if we start by examining the language which the average scientist uses when he tries to give students or laymen not only information about the phenomena and laws of science but also what one calls a ‘ real understanding ’. For example, in trying to explain the Law of the Conservation of Energy the teacher will not be satisfied by merely enumerating the physical experiments by which this law can be tested, for instance the measurements of temperature, etc., which are used for this purpose. Most teachers would point out, in addition, that the energy cannot ‘ disappear ’. This will seem comprehensible to the listeners since everyone knows what it means to say that a stone or a human being cannot ‘ disappear ’. The ‘ conservation of energy ’ is linked, in this way, to an everyday life experience with which it has very little to do from the viewpoint of logical analysis.

The term ‘ disappear ’ belongs to the language of fairy tales which we learned in the nursery. The physical law of conservation is interpreted by its analogy to the ‘ metaphysical ’ law that ‘ something cannot become nothing and *vice versa* '. If we look at the bearing of physical theories in a more general way we often find the opinion that the formulation by differential equations and their methods of integration may help us to predict and to handle phenomena but does not give us ' understanding '. Therefore, one resorts often to metaphysical interpretations. It is claimed that ' behind the phenomena and the equations ' there is some ' real world '.

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[1] G. B. Phelan, *St. Thomas and Analogy*, Milwaukee, 1941

As a matter of fact this ' reality ' is described by way of some analogy that uses our everyday language. One says, for example, that all physical phenomena are ' really ' mental occurrences. By this expression one links even the most complicated physical phenomena to what we have learned since our childhood to call ' mental '—for example, expected cake in contrast to an actual cake. Another way of interpretation is to say that the ' real ' behind all phenomena is a collision of hard bodies and the well-known effects of such collisions. In this case we interpret complicated and rare phenomena by very familiar experiences which we have known since our childhood. These interpretations have resulted in ' metaphysical creeds ' which are called ' idealism ' and ' materialism ', respectively. This simple remark leads us to the hypothesis that perhaps all so-called ' metaphysical interpretations of science ' have been attempts to make the general and abstract principles of science more intelligible. These principles have been linked with analogous statements which are expressed in a more familiar language. The highly abstract principles of science are interpreted in the language of metaphysics which is nearer the language of ' commonsense '. These interpretations are the ' facts ' which are investigated by metaphysics.

We shall try to back up this hypothesis in two ways. First, we shall examine historically how the metaphysical interpretations of science have gradually separated themselves from the formulations of science proper. Secondly, we shall examine the attitude of philosophers with scientific background who have advocated metaphysical interpretations. How did they distinguish between science itself and the metaphysical discourse by which they hoped to reach a deeper ' understanding ' of science ?

If science is interpreted in the language of everyday life, it is linked closely to the language by which we describe human behaviour. Hence, this ' metaphysical interpretation ' can be used for influencing human conduct. It can serve the ' practical aims ' of metaphysics. We shall touch this point in the present paper only lightly.

In ' science proper ' we derive from the abstract principles, by long logical chains, results about observable facts which can be linked with human behaviour. But the ' metaphysical interpretations ' avoid these long chains and link the ' abstract principles ' by a kind of ' short circuit ' directly with statements about human behaviour. It is a well-known fact that Aristotle and some medieval philosophers interpreted the orbits of the planets as perfect orbits produced by ' perfect spiritual beings ' which had to serve as examples for human conduct, as ideals which should be shining beacons but could never be reached.

In our own century the new physical theories, the theory of relativity and the quantum theory, have been formulated by their authors in a highly abstract language. Great efforts have been made by scientists and scientific writers to present these theories in such a way that the ' man in the street ' can acquire some ' understanding ' of these new paths in science. But quite a few scientists have regretted that the popularisers of modern science have frequently tried to bring about this ' understanding ' for the ' layman ' by stressing the metaphysical interpretations of these new theories. ' Serious scientists ' have required that science should be popularised by stressing merely the new facts which have been discovered and the new relations between these observable facts. A good ' populariser ' should refrain from teaching those dubious interpretations along with the truly scientific material, not to mention refraining from emphasising them.

There is certainly some ground for this complaint ; but, on the other hand, we must not disregard the fact that there is some genuine relationship between a good popularisation and a metaphysical interpretation of science. If we want to make a system of abstract principles (like relativity or quantum theory) intelligible to a layman we have to explain it to him by some analogy which is expressed in commonsense language and which has, if possible, some application to human behaviour. As we learned, the metaphysical interpretations of science stress similar analogies. Therefore, we can say that in a certain way the metaphysical interpretations of science are a sort of popularisation of science and have to be so.

2 *The Distinction between Science and its Metaphysical Interpretations : its Historical Background*

If we want to understand the distinction between principles which are accepted on scientific grounds, in the modern sense, and the metaphysical interpretations of these principles, it is advisable to remember that this distinction has not always existed. A. N. Whitehead describes in a way which is both precise and impressive how science was born. He writes,[1] ' All the world over and at all times there have been practical men, absorbed in " irreducible and stubborn facts " ; all the world over and at all times there have been men of philosophic temperament, who have been absorbed in the weaving of general principles.' But it is, according to Whitehead, the ' union of passionate interest in the detailed facts with equal devotion to abstract generalisation which forms the novelty in our present society. Previously it had appeared sporadically and as if by chance.' But in our society this ' balance of mind ' has become ' part of the tradition which infects cultivated thought.'

Before this amalgamation the practical work in engineering, building or artillery was done according to traditions which had been transmitted in the profession. They were little co-ordinated with the scientific theories which had been developed, e.g. in the ' physics ' of Aristotle. Because these philosophical systems did not claim to give technical advice to the engineer or artisan, they could devote themselves completely to the task of ' understanding ' the phenomena of nature, leaving the technical application to a socially ' lower ' type of men. ' Science ' and ' Philosophy ' were nearly one and the same thing or, to use the language of this paper : science and its metaphysical interpretation were nearly one and the same thing. We can understand this statement very well by reading how Aristotle describes the procedure of what he called ' physics '. He writes,[2] ' The natural path of investigation starts from what is more readily knowable and more evident to *us* and proceeds to what is more self-evident and intrinsically more intelligible ; for it is one thing to be knowable to us and quite another thing to be intelligible objectively.'

This procdeure includes science, which is knowable by sense perception ' to us ', and also its metaphysical interpretation, which is supposed to be ' self-evident and intrinsically clear ' and based on ' objectively intelligible principles '. One is satisfied in modern science if one can derive from the principles observable facts, but the ancient philosophers or scientists required, in addition to this, that those general principles had to be ' intelligible '. While in modern science the main function of the principles is a ' practical ' one, in antiquity they had, above all, to provide satisfaction to our desires for ' intelligible causes '. This union between science and metaphysics remained alive through the Middle Ages and was at the basis of Descartes' conception of science.

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[1] Whitehead, *Science and the Modern World*, Cambridge, 1926, Chapter I

[2] Aristotle, *Physics*, Book I

In the preface to the French edition of his *Principles of Philosophy*,[1] Descartes compares philosophy with a tree. The roots are metaphysics, the trunk is physics and the fruits on the branches are applied science—medicine, mechanics and ethics.

This union of science and its metaphysical interpretation was preserved as long as science was stressing the construction of intelligible principles and was minimising the derivation of technical knowledge. Perhaps the only field in which a large body of observable facts was already derived in antiquity by logical conclusions from general principles was astronomy. As a matter of fact in this field the split between science and metaphysics had occurred by the time Greek philosophy had reached its peak.

There was, on the one hand, the doctrine that all celestial bodies move in circles with uniform velocities. This was ' intelligible ' because this was regarded as the most perfect and most dignified kind of motion. This motion was in accord with the nature of the sun and the planets, which were regarded as divine. On the other hand, it was very well known to the Greek astronomers that the positions of the sun and the planets, calculated on these assumptions, were not in good agreement with the observed positions on the sphere if one required a reasonable degree of precision. The scientists and philosophers of antiquity and the Middle Ages were quite familiar with these two aspects of astronomy and actually two different systems of knowledge were built up along two different lines and with two different criteria of truth.

One system, which we could call ' science in the modern sense ', emphasised agreement with the results of observation. The other system, which we may call ' metaphysics ', was satisfied with a poor agreement with observation and emphasised the derivation from intelligible principles.

This distinction was very clearly formulated by Plato. In chapter VII of his *Republic* he discusses the curriculum of studies for the future ' leaders ' of his ideal state. The question arises whether these candidates for leadership should study astronomy. Plato stresses the point that for the preparation of a young man for a government career the metaphysical aspect of astronomy is of much greater importance than the scientific one. The observable results of astronomy are useful in agriculture or navigation ; but these fields of knowledge are not so important for the future rulers as the art of effectually influencing human behaviour. The metaphysical aspect teaches us to infer from the perfect orbits of the celestial bodies that they are moved by divine beings. Therefore astronomy, in this interpretation, can be used as a support for the belief in divine beings and is therefore useful in the education of good citizens. Hence, according to Plato, the future ruler has to learn astronomy with the emphasis upon its metaphysical interpretation. This kind of interpretation is, to a high degree, independent of any advance in science. Scientific astronomy could not uphold the theory of circular motion since scientific statements are to be checked by observation. However, the metaphysical interpretation has never claimed to check precisely with observations. Therefore, it has never been forced to abandon the theory of circular motion. Scientific astronomy has been developed by generations of scientists more or less independent of the existing system of government, but the metaphysical interpretations have been susceptible to a high degree to any influence of political powers. Therefore, since the time of Plato, any government which believed strongly that it knew what is good or bad for the citizen would give strong support to the metaphysical interpretations which it regarded as favouring a 'good' conduct of men.

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[1] Descartes, *Principles of Philosophy*. Original edition in Latin, 1644. French translation, 1647

As early as in the writings of Thomas Aquinas we meet hints that this split is not restricted to astronomy but has widened to a deep gap within the system of what was called 'science' or 'philosophy'. As a matter of fact Thomas Aquinas realised that the two aspects of astronomy, the scientific and the metaphysical one, are two general types of human approach to knowledge which correspond to two different criteria of truth. He says :[1]

> Reason may be employed in two ways to establish a point : first, for the purpose of furnishing sufficient proof of some principle, as in natural science, when sufficient proof can be brought to show that the movement of the heavens is always of uniform velocity. Reason is employed in another way, not as furnishing a sufficient proof of a principle, but as confirming an already established principle by

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[1] St. Thomas Aquinas, *Summa Theologica*, first part, 'The knowledge of divine persons'. First article, Reply, Objection 2

> showing the congruity of its results, as in astrology the theory of eccentrics and epicycles is considered as established because thereby the sensible appearances of the heavenly movements can be explained ; not, however, as if the proof were sufficient, in as much as some other theory may explain them.

There is hardly a text in the philosophical writings of the seventeenth, eighteenth, and early nineteenth century which would define the distinction between the scientific criterion of truth and the truth of a metaphysical interpretation in such a clear and subtle way. This passage, which does not fit very well into the general climate of Thomistic philosophy, anticipates in some way the new logic of science which was advanced in the last quarter of the nineteenth century by such men as Mach, Pearson, Duhem and Poincaré. As a matter of fact, the passage from St. Thomas which we quoted played a great part in forming the opinions of Pierre Duhem, in the logic of science as well as in the history of science. St. Thomas characterised very correctly what is today the accepted criterion of truth in science and pointed out that by this criterion the truth of a theory can never be ' proved '. Every confirmation by experiential test proves only that the theory is not refuted by the special experiment in question. St. Thomas stresses the point that ' truth ' in the philosophical sense cannot be confirmed by the method of confirmation that is accepted in modern science. By using the word ' science ' in this modern sense, the union of science and philosophy cannot be preserved. Besides the ' philosophical truth ', St. Thomas introduces a kind of ' inferior ' type of truth which is what we call today the scientifically confirmed truth.

In the later scholastic philosophy,[1] it was pointed out more and more that even a great many statements used in ' sacred theology ' can only be proved by this ' inferior ' method. We can, therefore, have no certainty about their validity from the light of human reason. At that time, of course, the conclusion was that the certainty has to come from a support which is provided to the weak scientific method by divine revelation.

If there are different scientific hypotheses which are in agreement with the observed facts, no scientific method can decide which hypothesis is ' true '. According to medieval philosophy the decision must come from metaphysics and if human reason is not able to reach a metaphysical decision, this decision can only come from divine revelation. It was quite common in medieval philosophy to say that only God can know whether the Ptolemaic theory of epicycles is the ' true ' cause of the observable motion of the planets.

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[1] By Roger Bacon, William of Ockham, and the school of Nominalism in general

This view later played a great role in the dispute between the Ptolemaic and Copernican systems. We understand in this way the famous lines in Milton's *Paradise Lost* [1] when Adam consults the Archangel Raphael about the new Copernican system. The angel answers that this question will never be decided by human beings and is reserved to God.

It is interesting that the Thomistic distinction between two criteria of truth was introduced by its author for a specific purpose, and this was a theological one. The existence of God can, according to St. Thomas, be proved philosophically by a metaphysical interpretation of our experience ; but the existence of the Trinity can only be confirmed by ' congruity ' of its ' consequences ' with our experience. Therefore, the existence of the Trinity cannot be ' proved philosophically '. The belief in the Trinity has—in the light of reason—only the certainty of a scientific theory in the modern sense, e.g the certainty of the belief in epicycles. Philosophical certainty for the belief in the Trinity could only be provided by divine revelation in the same way as for the belief in epicycles.

From this division between ' confirmation ' by checking the consequences and ' proof ' by derivation from ' intelligible ' principles, the division between ' science in the modern sense ' and its ' metaphysical interpretation ' has developed. The more science has advanced, particularly in the nineteenth century, the more the emphasis has been shifted towards ' yielding observable facts ' as the criterion for the validity of the principles. But then the formulating of the wording of these principles became less and less relevant. Eventually, it became unimportant whether the principles were ' intelligible ' or not.

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[1] for Heav'n
Is as the Book of God before thee set,
Wherein to read his wondrous Works, and learn
His Seasons, Hours, or Days, or Months or Years :
This to attain, whether Heav'n move or Earth,
Imports not, if thou reck'n right, the rest
From Man or Angel the Great Architect
Did wisely to conceal. . . .'
Paradise Lost, Book vii

On the other hand, the less attention science paid to the ' intrinsic plausibility ' of its principles, the more metaphysical systems developed and the more they stressed their independence of science. They claimed to start where science had stopped. One can say with a certain justification that the separation was completed about 1800.

It is perhaps nowhere dramatised as strongly as in the writings of the leading German philosopher, Immanuel Kant, in his *Critique of Pure Reason.* Kant raises the problem of whether the most important metaphysical problems, the freedom of the will, the existence of an immaterial soul and the existence of God can be solved by the use of human reason. Kant declares bluntly that, so far as the logical derivation of observable facts is concerned, it does not make any difference whether we assume the freedom of the will or not, whether we believe in an immaterial soul or not. He says in his *Critique of Pure Reason*[1] ' Reason is impelled by a tendency of its nature to get beyond the field of experience and to venture . . . . by means of mere ideas to the utmost limits of all knowledge. . . . But whatever discoveries may be made, they can never be applied to facts, i.e. in scientific research.'

According to Kant, metaphysical statements are not the result of reason ' in its use for speculative purposes ', but these metaphysical results serve the ' practical interests of reason '.

Kant put to himself the question why some metaphysical statements (e.g. that the will is free) ' are so strongly recommended to us by our reason ' although they cannot be applied to observable facts. His answer is that ' their importance is probably connected with the practical '. This means obviously that they are helpful in influencing human behaviour. We see, according to Kant, that a speculative or theoretical picture of the world is produced only by science and not by its metaphysical interpretation which serves rather practical purposes. This view is, in its practical consequences, very near to the way in which an American pragmatist like John Dewey looks at metaphysics. Dewey says,[2] ' The history of philosophy will take a new significance. What is lost from the standpoint of would-be science is regained from the standpoint of humanity. Instead of the disputes of rivals about the nature of reality, we have the scene of human clash of social purposes and aspirations.'

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[1] Kant, *Critique of Pure Reason*, English translation by Max Mueller, 1927, p. 640 ff. The passage quoted above is partly translated directly from the German text.

[2] J. Dewey, *Reconstruction in Philosophy*, New York, 1920 ; enlarged edition, Boston, 1948

We see that after the break between science and metaphysics had become complete the now isolated metaphysics was serving two purposes. The first, the more theoretical one, was to give to the abstract principles of science an interpretation in terms of a commonsense language, for example by stressing the analogy with well-known mechanisms or organisms. This kind of interpretation will be elaborated more and more in this paper. The other task was to serve as an instrument for shaping human behaviour.

The history of the nineteenth and twentieth centuries shows two main reactions to the completed separation of metaphysics from science. On the one hand, this separation was hailed and the split widened more and more. Attempts to renew Thomistic metaphysics have been made and a strong neo-Thomistic movement has arisen. We note also the rise of new metaphysical systems like those of Bergson, Whitehead, Heidegger and Sartre.

On the other hand, since the middle of the nineteenth century we note strong attempts towards a new reunion of science and philosophy. The new system would no longer be based on metaphysics, but on science. The split was to disappear by building bridges and using scientific material. We have to remember the attempt of Comte to eliminate metaphysics altogether and the attempt of Herbert Spencer to restrict metaphysics to a small marginal area, the 'Unknowable'.[1] In America, the 'Cosmic Philosophy' of J. Fiske[2] was based upon the ideas of Spencer and Comte; it shared with Spencer the injection of a marginal metaphysics.

It is well to remember how Comte defined the method of science in contrast to metaphysics.[3] He defined his 'positive philosophy', which was to integrate all human knowledge by scientific methods, as 'the theory, the object of which is the co-ordination of observed facts, whatever system of ideas may be employed for this purpose.' Comte stresses as the characteristics of 'science' in contrast to metaphysics that science attempts a 'co-ordination of observed facts' and that it does not care 'whatever system of ideas may be employed for this purpose'. This means that science regards the kind of concepts which are used in the formulation of its principles as irrelevant.

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[1] H. Spencer, *First Principles*, 1860

[2] J. Fiske, *Outline of Cosmic Philosophy*, New York, 1874

[3] A. Comte, *Positive Philosophy*, Ch. I

It rejects the procedure of science which was prescribed by Aristotle : the search for ' intelligible principles '. Science is not interested in whether its principles are ' intrinsically knowable and intelligible '. After the separation this task is left to metaphysics, which becomes now an interpretation of science in an ' intelligible language '. It will turn out more and more that there is no other ' intelligible language ' than the ' language of common sense ' in which we have learned to express—since our childhood—the experience of our everyday life.

Philipp Frank

(Part II will be published in the next number.)

BOOKS RECEIVED FOR REVIEW

Inclusion of books in this list does not preclude their being reviewed in later issues

John Read, *The Alchemist in Life, Literature and Art,* Thomas Nelson & Sons Ltd., Edinburgh, 1947, pp. xii + 100, 10s. 6d.

H. Dingle, *Science and Literary Criticism,* Thomas Nelson & Sons Ltd., Edinburgh, 1949, pp. 184, 7s. 6d.

L. L. Whyte, *The Unitary Principle in Physics and Biology,* The Cresset Press, London, 1949, pp. ix + 182, 12s. 6d.

Alfred Hooper, *Makers of Mathematics,* Faber & Faber Ltd., London, 1949, 18s.

H. Butterfield, *The Origins of Modern Science 1300–1800,* G. Bell & Sons, London, 1949, pp. x + 217, 10s. 6d.

Franklin J. Matchette, *Outline of a Metaphysics,* The Philosophical Library, Inc., New York, 1949, pp. xiv + 108, $3.75.

W. M. Urban, *Beyond Realism and Idealism,* George Allen & Unwin Ltd., London, 1949, pp. 266, 18s.

Sir Edmund Whittaker, *From Euclid to Eddington,* Cambridge University Press, 1949, pp. ix + 212, 15s.

E. F. Caldin, *The Power and Limits of Science,* Chapman & Hall Ltd., London, 1949, pp. ix + 196, 12s. 6d.

Moritz Schlick, *Philosophy of Nature,* The Philosophical Library, Inc., New York, 1949, pp. xi + 136, $3.00.

H. McLachlan, *Sir Isaac Newton—Theological Manuscripts,* selected and edited, with an Introduction. University Press of Liverpool, 1950, pp. vii + 147, 7s. 6d.

Richard Robinson, *Definition,* Clarendon Press, Oxford, 1950, pp. viii + 207, 15s.

Gilbert Ryle, *The Concept of Mind,* Hutchinson's University Library, London, 1949, pp. 334, 12s. 6d.

R. C. Stauffer, *Science and Civilisation*—edited by R. C. Stauffer. University of Wisconsin Press, Madison, U.S.A., 1949, pp. xiii + 212, $2.50.

Fr. Vinding Kruse, *The Foundation of Human Thought,* Oxford University Press, London (Einar Munksgaard, Copenhagen), 1949, pp. 404, 30s.

Hermann Weyl, *Philosophy of Mathematics and Natural Science*—revised and augmented English edition, based on a translation by Olaf Helmer. Princeton University Press, 1949 (London, Geoffrey Cumberlege), pp. x + 311, 40s.

REVIEW

The Structure of the Universe, G. J. Whitrow, Hutchinson's University Library, London, 1949. Pp. 171. 7s. 6d.

Historians and philosophers of science may perhaps look to Dr. Whitrow's book to provide an account of the present state of the purely scientific side of his subject, rather than to contribute to their own special fields. It may be said at once that he has given a brilliant description of the astronomical evidence concerning the structure, behaviour and ages of the galaxies and concerning the structure of the whole system of galaxies that constitutes the observable universe. He has given also an extraordinarily able (non-mathematical) description of current general cosmological theory, with special prominence for the ideas of Eddington and Milne, together with some account of more detailed matters such as galactic kinematics and dynamics.

Dr. Whitrow is, however, particularly well qualified to write also upon both the historical and philosophical aspects of his subject : readers specially interested in these aspects will be glad to find that his presentation keeps them so well in view. In fact, even the professional historian of science will get a fresh perspective in his own domain from the material brought together by the author in his first three chapters, which give a historical account of the exploration of the universe and of ideas of space and time.

Philosophical readers, as such, will be particularly interested in Dr. Whitrow's concluding chapter on ' Cosmology and the *A Priori* '. This could be expanded into a book by itself—and Dr. Whitrow is the author one would wish to see producing such a book. Nevertheless, in the space at his disposal in the present work, he succeeds in giving a surprisingly full critical summary of the philosophical positions adopted by Eddington and by Milne, and he is able also to make some highly suggestive comparisons between their systems.

Dr. Whitrow rightly recalls the claims that have been stated by these authors themselves. In the context he might, however, have shown how their work does appear to fit naturally into the development of physical theory, if the work be viewed perhaps a little apart from what they themselves claim for it.

Classical physics was formulated with no explicit mention of the *observer*. Relativity theory was the first expressly to involve the observer, but to no greater extent than that any physical property is regarded as describable only by reference to a specified observer ; the property is regarded as uninfluenced by the act of observation. Quantum theory involves the observer much more intimately: its formulation necessitates that the act of observation should have an unavoidable effect upon the system observed. A physical phenomenon becomes a property neither alone of the system observed nor alone of the observer, but of the two taken together. Eddington's theory may be viewed as an attempt to take what appears to be the natural next step and to regard physical phenomena as almost entirely properties of the observer and to credit the system observed with little more than existence. There are admittedly great differences of opinion regarding Eddington's success, but the evolution of ideas, traced so extensively in Dr. Whitrow's book, seem to lead almost inevitably to the attempt being made.

Generally speaking, Milne's work may be regarded as another such attempt. Dr. Whitrow gives an illuminating analysis of its difference from Eddington's and tentatively suggests that the two 'may provide complementary perspectives of the physical universe', and ascribes this, even more tentatively, to the fact that 'Eddington's concept of physical measurement is associated with the idea of the ruler, Milne's with the idea of the clock'. This apparently simple comment seems to the reviewer to penetrate very deep; for the concepts of *measurement* associated with the two ideas are fundamentally different and a satisfactory physical theory must depend upon adequate analyses of both concepts. Such analyses may well prove to be much like those formulated by Eddington and Milne. But it also seems to the reviewer that the two ideas may not correspond, as Whitrow suggests, to the spatial and to the temporal aspects of the universe, but rather to the material and to the spatio-temporal aspects, respectively. This view is somewhat borne out by the fact that Eddington's analysis led him to discuss the fundamental properties of matter, whereas Milne's led in the first place to a discussion of the universe in the large. The view does not depend, however, upon the correctness or otherwise of these discussions.

Whatever the possibilities of what Whitrow classes as *a priori* theories, their present limitations are emphasised by an hypothesis advanced (in definite form and in accessible accounts) only after his book was written. It is that of the continuing creation of matter as formulated by Jordan, Bondi and Gold, and Hoyle. The hypothesis may not be 'true', but it represents a conceptual possibility about which it is exceedingly difficult to see how these theories can make any pronouncement. This is not to criticise Whitrow's analysis: indeed it seems merely to underline many of his arguments.

Sufficient has perhaps been written to show that Dr. Whitrow's book, modest though it be in appearance, is a quite exceptionally stimulating piece of work.

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